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Migrant Opportunity and the Educational Attainment of Youth in Rural China

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Abstract

This paper investigates how the reduction of barriers to migration affected the decision of middle school graduates to attend high school in rural China. Change in the cost of migration is identified using exogenous variation across counties in the timing of national identity card distribution, which made it easier for rural migrants to register as temporary residents in urban destinations. After taking care to address potential strengths and weaknesses of our identification strategy, we find a robust, negative relationship between migrant opportunity and high school enrollment that cannot be explained by geographic convergence in access to education across rural China.

This paper—a product of the Human Development and Public Services Team, Development Research Group—is part of a larger effort in the department to study the effects of rural-urban migration on household outcomes and investment decisions in migrant sending communities. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at jgiles@worldbank.org.

Produced by the Research Support Team

Migrant Opportunity and the Educational Attainment of Youth in Rural China[♠]

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1. Introduction

The ability to migrate, both internationally and within national borders, is generally associated with improved living standards of migrant families and others in both migrant and local destinations. Beyond benefits that migration may have for raising the incomes, well-being and risk-coping ability of families, a growing literature examines the direct and indirect effects of migration on decisions to invest in education and on educational attainment. Migration may facilitate investment in education as remittances from migrants ease credit constraints (Yang 2008), or alternatively, higher returns to education in migrant destinations may alter the perceived returns to investing in education (Beine, Docquier and Rapoport 2008). Does increased migration necessarily lead to more investment in education? Recent evidence from the international migration literature suggests that this is not necessarily the case. When migrants capable of providing referrals have less skill and are employed in occupations requiring less education, then reducing the cost of finding low skill migrant employment may be associated with a decline in educational investments (McKenzie and Rapoport 2011).

In this paper, we focus on identifying the net effect of the migrant labor market on the decisions of families in rural China to enroll middle school graduates in high school. While middle school completion is mandated by policy (Tsang 1996), high school education is neither compulsory nor as heavily subsidized as at the primary and middle school levels. As high school tuition can be a substantial share of household annual income, migrant income from household members or relatives may relax credit constraints and facilitate investment in education beyond middle school. This effect has been documented in other settings: increased wealth associated with migrant or other off-farm employment opportunities has eased credit constraints and led to higher enrollment rates (Edmonds 2004; Glewwe and Jacoby 2004; Yang 2008). In addition, if returns to high school are significantly higher than middle school, either locally (Foster and Rosenzweig 1996) or in migrant destinations (Kochar 2004; Beine, Docquier and Rapoport 2008), one might expect to observe an increase in the probability that families will enroll children in high school.

While increases in migration from a village may relax credit constraints affecting high school enrollment, the lower associated costs of finding a job raise the net return to migrant employment and therefore the opportunity cost of remaining in school. If an increase in the size of the migrant network lowers barriers to employment in the low skill jobs typically held by rural migrants, the perceived returns to an additional year of school may increase less than the opportunity cost of remaining in school. In this paper, we find that expansion of the migrant network leads to a decrease in the probability that children born in rural China will attend high school.

Our finding has implications beyond China as it highlights the role that information signals from social networks of less-skilled migrants may play in influencing educational investment in the next generation. Further, it should be noted that this economically significant effect need not be the direct result of immediate participation of middle school graduates in the migrant labor market. As migration increases, the local working age population declines, which could lead to an increase in the relative return to unskilled labor locally. In addition, high school age children may wait to migrate until they are of age to apply for national ID cards (lowering risks associated with migrant work) and have obtained information about employment prospects outside their home counties.

The paper develops an instrumental variables (IV) approach useful for studying the effects of migration on a range of outcomes in source communities of rural China from the 1990s through the mid-2000s, and contributes to literatures examining the effects of rural to urban migration, and migration more generally. Our IV approach uses a reform in the residential registration system that made it easier for rural migrants with national identification cards (IDs) to live legally in cities after 1988. National IDs, which were first available to urban residents in 1984, were not available in all rural counties as of 1988. We show non-parametrically that the share of the registered village resident working-age population working and living as migrants is a non-linear function of years since residents of a county received IDs, and then exploit differences in the timing of ID distribution to identify the effects of migration. To address concerns that the timing of ID card distribution may be associated with unobservables related to migration decisions, we first demonstrate that timing is unrelated to exogenous agro-climatic shocks affecting earnings in the local economy. We then present evidence that the timing of ID distribution is not systematically related to either time-varying local policies, which could influence the desirability of both high school enrollment and migration, or time-varying proxies for local administrative capacity, which could be related to village leader responsiveness to local demand for IDs.

After showing that migrant opportunity has a negative effect on high school enrollment, we examine whether there are heterogeneous effects that vary with parent background. Migrant opportunity has a stronger negative effect on the high school enrollment of children from families in which parents are cadres, small scale enterprise owners or managers, have some high school education, or had significant off-farm work experience. Given that children from such families are otherwise more likely to attend high school, finding a stronger effect among these groups provides evidence that we do not simply identify the decisions of families who remain unlikely to enroll children in high school after access to education improves in more rural communities.

Finally, we examine spillover effects of increases in migration on local employment. A larger migrant network outside the village implies a smaller local labor force. As migration from the village increases, the opportunity cost of high school may rise because the net return to migrant employment is increasing and because a shrinking local labor force leads to an increase in the relative returns to local employment. As the migrant labor force increases in size, the probability that high school age children find either migrant or local wage employment both increase, but there is no evidence that youth leave school to work in household agricultural activities or family-based non-agricultural enterprises.

2 Background

A. Rural-Urban Migration in China

During the 1990s, China's labor market changed dramatically with rapid growth in the population of rural migrants moving to work in urban areas. Estimates using the one percent sample from the 1990 and 2000 rounds of the Population Census and the 1995 one percent population survey suggest that the inter-county migrant population grew from just over 20 million in 1990 to 45 million in 1995 and 79 million by 2000 (Liang and Ma 2004). Surveys conducted by the National Bureau of Statistics (NBS) and the Ministry of Agriculture, which include more detailed information on short-term migration, suggest that there were well over 100 million migrants by the early 2000s (Cai, Park and Zhao 2008).

In other settings, referral through networks of earlier migrants is found to be an important feature of the job search and migration decisions of potential migrants (Montgomery 1991; Carrington, Detragiache and Vishnawath 1996; Munshi 2003). In China, Rozelle et al. (1999) emphasize that villages with more migrants in 1988 experienced more rapid migration growth by

1995. Zhao (2003) shows that the number of early migrants from a village is correlated with the probability that an individual without prior migration experience participates in the migrant labor market. Meng (2000) also suggests that variation in the size of migrant flows can be partially explained by the size of the existing migrant population in potential destinations.

The experience of migrants confirms the importance of networks for finding employment in urban areas. In a survey of rural migrants conducted in five of China's largest cities in late 2001 more than half of the migrant respondents had secured employment before their first migration experience, and over 90 percent moved to an urban area where they had an acquaintance from their home village. Notably, more than half of migrants surveyed had a member of their extended family living in the city before migrating, and over 65 percent knew hometown acquaintances other than a family member in the city (see summary statistics in Appendix Table A.1).

B. The Rural Educational System and the Educational Attainment of Rural-Urban Migrants

After passage of the Law on Compulsory Education in 1986 (Tsang 1996), children were required to complete six years of elementary and three years of middle school. In practice, some rural areas took considerable time to meet this standard, and as of 2004 many rural areas still provided only five years of elementary education instead of the mandated six years. Thus, middle school graduates in some rural areas complete eight years of formal schooling, while in other areas they complete nine years. After middle school, children may sit for examinations to enter academic or vocational-technical high schools, but families of students who pass examinations are required to pay substantial tuition for high school.

Prior research on rural-urban migrants has found a positive correlation between completed schooling and the ability to participate in migrant labor markets, and more generally returns in both goods and factor markets (e.g., Yang 2004). Much of this correlation, however, reflects the effect of increasing education from low levels to completion of compulsory middle school education. Indeed, a considerable body of descriptive evidence shows that most migrants have middle school education or less. Nationally representative statistics on educational attainment among migrants from the 2003 National Bureau of Statistics (NBS) Rural Household Survey, highlighted in Table 1, show that just over 85 percent of rural migrants nationally had a middle school education or less. Moreover, a much higher share of working-age adults who work in local non-agricultural activities have a high school education (21 percent) than adults engaged in migrant activities (14 percent). In contrast with rural residents, nearly all children born in urban areas complete high school education, and a substantial and growing share continue on to university or post-secondary vocational-technical training programs.

C. Evidence on Educational Attainment and Age of Migration from the RCRE Supplemental Survey

For our primary analysis, we use household and village surveys conducted in collaboration with the Research Center for Rural Economy (RCRE) at China's Ministry of Agriculture. Conducted in 52 villages located in four provinces, all 3999 households in the 2003 wave of RCRE's household panel for these four provinces were enumerated between August and October 2004, allowing us to match households and villages from the 2004 supplemental survey with a household and village panel survey conducted annually by RCRE since 1986.[1] A unique feature of the supplemental survey is that it enumerated the educational attainment, birth year, current occupation, work and migration history, and residence location for all children and other current and former residents (including deceased former residents) of survey households. The survey design allows us to avoid the selection bias that would occur if we used household survey data that only includes current household residents to study educational attainment. One might yet be concerned that entire families may migrate to locations with better education, but this was a rare occurrence prior to the mid-2000s.

Figure 1 summarizes trends in high school enrollment by cohort for individuals born after 1940 and residing (or previously residing) in RCRE households. Consistent with research using the population census (e.g., Hannum et al. 2008), the supplemental survey data also show that educational attainment rose steadily over time, with the average educational attainment of girls and boys converging by the 1975 birth cohort (Appendix Figure A.1). Summary statistics on the age of first migration are consistent with findings from surveys of migrants in urban areas. Between 1987 and 2004 the number of migrants of all ages increased while the average age at

[1]A detailed discussion of a larger nine-province sample from the RCRE panel dataset (The National Fixed Point Survey), including discussions of survey protocol, sampling, attrition, and comparisons with other data sources from rural China, can be found in the data appendix of Benjamin, Brandt and Giles (2005), and source documents on sampling, survey design and survey instruments are available at: https://sites.google.com/site/decrgjohngiles/datasets. This paper makes use of village and household data from the four provinces (Anhui, Henan, Jiangsu and Shanxi) where the authors conducted follow-up household and village surveys in 2004.

first migration remained fairly constant at 20 years of age (Figure 2). Individuals over 30, however, might reasonably be considered outliers who keep the average age of migration constant when it would have otherwise declined due to increasing migration among young adults and teenagers. Figure 3 shows *lowess* estimates of the share of three teen cohorts engaged in temporary or long-term migrant employment outside of their home counties. While the share of 15 and 16 year old children in migrant employment is increasing, the rate of increase does not appear dramatic, and the level of migration is low enough that it would not necessarily require a decline in high school enrollment. As teenagers were required to be 16 years of age before applying for national IDs, which facilitated migration, and processing times to obtain IDs could take several months, it is not surprising that the migration rate of 15 and 16 year olds is relatively low. Consistent with waiting times both for IDs and referrals for first jobs, the shares of 17 and 18 year olds and 19 and 20 year olds working in migrant jobs increase at a much faster rate.

3 Understanding the High School Enrollment Decision

A. Theoretical Background and Evidence on Signals from the Urban Labor Market

Behind our empirical analysis of how migration affects the high school enrollment decision lies a simple theoretical model, similar to Glewwe and Jacoby (2004) and Kochar (2004), highlighting potential positive and negative effects of migration on the educational investment decision. Briefly, when off-farm income increases, a wealth effect may ease credit constraints that prevent families from paying high school tuition, and may facilitate high school enrollment. Second, the expected return to schooling may increase with the ability to earn higher income in the migrant labor market and this would also raise the probability of enrolling in high school. Alternatively, the probability of continuing on to high school may decrease if potential migrants receive information and referrals from earlier migrants, who typically have only middle school education. Because wages for middle school graduates will be higher in migrant destinations, the flow of information about jobs from migrants will mean that the perceived earnings ability for middle school graduates will increase relative to those completing high school. The value of middle school graduate time will increase, raising the opportunity cost of schooling, and decreasing the likelihood of enrollment in school. From a theoretical perspective, the net effect of migration on the high school enrollment is thus indeterminate.

Numerous studies suggest that returns to education in urban China are non-linear in educational attainment and that through the mid-2000s, the rising returns to education in urban China were driven by high returns to college education (Heckman and Li 2004; Cai, Park and Zhao 2008). Most studies suggest a significantly lower return to a year of high school education, and Li et al. (2012) even suggest that the return to high school in urban China was not significantly different from zero. Moreover, as documented by Meng and Zhang (2001), rural migrants faced limited opportunities in higher skill occupations as many cities explicitly reserved some occupational categories for urban residents, and even where this practice was relaxed by the early 2000s, there was often *de facto* segregation of rural residents into unskilled service and construction sectors, or into other relatively low skill jobs unwanted by urban residents. Finally, evidence from the RCRE survey suggests that migrants earn a significant return to education through middle school attainment, but nearly zero returns to completion of high school education.

From the standard theoretical framework, and the environment in which rural households are making education investment decisions, a reduced form version of the enrollment demand function can be written as:

$$E_t^* = E^*(\lambda_t, \mu_t, \psi_t, \theta_t, \mathbf{X}_{ht}, M_{jt}, P_t^e) \quad (1)$$

where enrollment demand in period t, E_t^*, is a function of the shadow price of capital, λ_t, which captures credit constraints, the expected return or shadow price of schooling, μ_t, child ability, ψ_t, productivity shocks, θ_t, household endowments and characteristics, $\mathbf{X}_{ht}$, migrant opportunity, as proxied by the share of village *j*'s labor force that is employed and living outside the home county, M_{jt}, and the tuition and other costs associated with high school enrollment, P_t^e. A full model highlighting the potential impacts of an increase in migrant opportunity, acting through changes in the shadow prices of physical and human capital, can be found in de Brauw and Giles (2006).

B. Empirical Approach

To understand how migrant opportunity affects the decision to enroll middle school graduates in high school, we need to control for such factors as lifetime wealth, preferences, prices and unobserved ability that might vary with both the probability of school enrollment and off- farm opportunities. From arguments of the enrollment demand function in equation (1), we write the reduced form model of the discrete decision of household *h* to enroll child *i* in high school:

$$(2) \qquad E_{ihjt} = \beta_0 + \beta_1 M_{jt} + \boldsymbol{Z}'_{jt}\boldsymbol{\beta}_2 + \boldsymbol{X}'_{hjt}\boldsymbol{\beta}_3 + \boldsymbol{u}_j + \boldsymbol{p} \cdot \boldsymbol{T}_t + \nu_i + e_{ihjt}$$

where E_{ihjt} is 1 if an individual completing middle school in year t enrolls in high school the following year, and 0 otherwise. As M_{jt} is drawn from a separate annual survey of village leaders and accountants, it is not constructed exclusively from survey households, and includes information on registered residents who have lived outside the village for a considerable period of time.[2] $\boldsymbol{Z}_{jt}$ are other time-varying characteristics of village j that potentially affect local returns to high school and alternative activities (the shadow value of schooling, μ_t, in equation (1)), and local factors influencing credit constraints faced by all households. These variables include the average village income per capita and Gini coefficient, both calculated leaving out the income of household *h*, the total arable land available in the village, land inequality (as measured by the cultivated land Gini coefficient), the size of the village labor force (the registered population aged 16 to 60), and the arable share of total village land. Household characteristics, $\boldsymbol{X}_{hjt}$, are introduced in some specifications to control for family preferences for education, factors affecting lifetime household wealth, and the likelihood that the household faces credit constraints. To proxy for family wealth, we include variables for parental education, which are time invariant, and for preferences, a time-varying measure of the number of ever-resident male and female children of the household head who have reached 16 years of age, which we refer to as *potential migrants*. Next, we include a vector of village fixed effects, $\boldsymbol{u}_j$, in all models because important village characteristics, like location, do not vary over time but influence both labor market returns and the cost of obtaining education. Price levels, macroeconomic shocks

[2]In each annual village survey, village leaders were asked about population of legally registered residents, and the numbers of legally registered residents who were presently living and working outside the village for more than six months of the year. We consider a registered resident who lives and works outside their home county to be a migrant. As an alternative to using the migrant share, one might alternatively use the number of village residents working as migrants outside the village, as the size of the migrant network may be more important than the migrant share of the village workforce. Residents from larger villages, with more migrants for a given share, may have advantages of a larger network, though there is also likely a point at which increasing village size has a negative effect on referrals. There are no qualitative differences to results presented in the paper if we use the size of the migrant population as the measure of migrant network size; in fact, F -statistics on the first stage are consistently higher when using the number of village residents working as migrants. In this presentation, however, we favor the share as it makes intuitive sense for a regressor to be a share when estimating a probability.

and trends may also affect family income, the cost of education, and the demand for migrant labor, and we control for these effects using province-year dummy variables, $\boldsymbol{p} \cdot \boldsymbol{T}_t$.

The ability of individual middle school graduates, ν_i, is unobserved but important for high school enrollment decisions, and reflects the education productivity parameter, ψ_t, in equation (1). In particular, students must test into high schools and it is likely that examinations are more competitive where the local supply of spaces in high school is more constrained. In models that include household information, parents' years of schooling proxy for inherited ability and the quality of parent-child interactions, although other dimensions will remain unobserved. In order to identify the effect of migrant opportunity on enrollment decisions, instruments for the migrant share of the village labor force must be plausibly unrelated to unobserved individual ability.

The value of E_{ihjt} is determined by whether a child *i*, who is estimated to complete middle school in year *t*, decides to enroll in high school in year $t + 1$. Assigning the value of E_{ihjt} requires making two assumptions about the timing of school enrollment and years of primary school. Although the RCRE supplementary survey provides us with an individual's age and years of schooling completed by 2004, we do not know the precise age at which each individual started school, but evidence from the 1990 China population census and other surveys makes it clear that the modal age for rural children to start school during this period was seven. We make a second assumption regarding the age at which children normally make the decision to enroll in high school. This age varies depending on whether children are in villages with five- or six-year elementary schools, and whether children are deciding to enter high school after 8 or 9 years of schooling, respectively. These differences are straightforward to determine from the RCRE data, and stable over the years that older children and young adults in our sample were making high school enrollment decisions. We provide evidence from the population census and other surveys on both of these assumptions in Appendix A.1.

A final concern involves repeated years of schooling or skipped grades. Although the supplemental survey did not ask explicitly about repeats or skips, the protocol required respondents to report years of schooling completed and the common interpretation is to answer in terms of the level of schooling completed. Examination of surveys that explicitly ask about repeats suggests that they are not common. The China Center for Agricultural Policy survey discussed in Appendix A.1, for example, finds that only six percent of rural students repeat a grade, and less than one percent repeat more than one. As the initial enrollment age varies, a more pressing concern for identification is whether errors in estimated start time, and therefore the year of the enrollment decision, will lead to failure of the exclusion restriction. We return to this concern in the discussion of our identification strategy below.

C. Identification Strategy

Estimates of equation (2) using OLS would yield biased estimates because our proxy for the cost of migration, the share of the village labor force employed as migrants outside the county, reflects factors that influence both the demand for and supply of migrants from the village. A persistent disruption to the local economy, for example, could decrease the ability of parents to pay tuition while raising the relative return to migrant employment, inducing a negative relationship. On the other hand, a positive correlation between the number of migrants from the village and unobservables affecting high school enrollment could exist if increases in household wealth or expanded high school capacity (and lower test scores required for competitive admission) occurred simultaneously with growing access to migrant employment. To identify the effect of the migrant network and the higher net return from migration that comes with referral, we must find an instrumental variable correlated with the level of migration from the village but unrelated to unobserved individual, household and village factors affecting high school enrollment.

We make use of two policy changes that, working together, affect the strength of migrant networks outside of a home county but are otherwise unrelated to the demand for and supply of schooling. First, a new national ID card (*shenfen zheng*) was introduced in 1984. While urban residents received IDs in 1984, residents of most rural counties did not receive them immediately. Where IDs are available, residents were able to apply for an ID after their 16th birthday. Second, a reform of the residential registration system in 1988 made it easier for migrants to gain legal temporary residence in cities, but a national ID was necessary to obtain a temporary residence permit (*zanzu zheng*) (Mallee 1995). While some rural counties made national IDs available to rural residents as early as 1984, others distributed them in 1988, and still others did not issue IDs until several years later. The RCRE follow- up survey asked local officials when IDs had actually been issued to rural residents of the county. In our sample, about half of the counties issued cards in 1988 (25 of 52), but cards were issued as early as 1984 in one county and as late as 1996 in another. Note that IDs were not necessary for migration, and large numbers of migrants live in cities without legal temporary residence cards. However, migrants with temporary residence cards have a more secure position in the destination community, hold better jobs, and thus plausibly make up part of a longer-term network in migrant destinations.

ID distribution had two likely effects on job search among potential migrants after the 1988 residential registration (*hukou*) reform. First, the costs of migrating fell after IDs became available. Second, if the quality of the migrant network improved with the years since IDs were available, then the costs of finding migrant employment should have continued to fall over time. As demonstrated in Figure 4 for the RCRE villages, the share of the village labor force working as migrants should be a function of both whether cards have been issued and the years since cards have been issued. As the share of the potential network has an upper bound, we expect the years-since-IDs were issued to have a non-linear relationship with the share of the migrant labor force.

For an instrument based on ID availability to be plausible or even conditionally exogenous, it is important that village-level governments were not able to influence the timing of ID availability, and that availability was unlikely to be coordinated with school policy. The institutional fragmentation of China's line Ministries minimize the likelihood that ID distribution could have been directly coordinated with education policy, or indirectly related through other development policies. County-level offices of the Ministry of Civil Affairs, which was responsible for registering residents and distributing IDs, are distinctly separate from offices of the Ministries of Agriculture and Finance, which set policies affecting land, credit, taxation and poverty alleviation, and offices of the Ministry of Education, which sets and enforces policy related to curriculum, tuition and fees. Further, when processing IDs, representatives of the Ministry of Civil Affairs must retrieve birth records from local offices of the Ministry of Health, and these records were not yet in electronic form for individuals in our sample.

Pressure on county-level governments by residents from individual villages who wanted IDs is unlikely given the large number of village-level units within each county. County-level units are comprised of township-level units, which govern villages. In 2005, China had 2853 county-level units, 40,466 township-level units and 704,386 village-level units.[3] As ID distribution was a county responsibility, and as the responsible office was in a different ministry from those directly affecting economic policy and education, we believe it is plausible that the timing of county-level ID distribution is unrelated to other policies and market conditions that may independently influence both migration from villages and the decision of village residents to enroll in high school. Because the presence of bureaucratic silos does not "prove" that the timing of ID availability is a valid instrument, we next look for evidence of violations of the exclusion restriction, and then further examine the effects of migration on high school enrollment using a plausible alternative instrument in Appendix A.2.

An initial concern is that the years-since-IDs were issued may simply be related to growth or other dimensions of the village economy. To evaluate this possibility, we initially demonstrate how the years-since-IDs were distributed varies with the share of the village working as migrants (Table 2). In column (1) we regress the share of village residents employed as migrants on dummy variables for each year after the distribution of IDs (nineteen in total) and four pre-distribution dummy variables (the year before, two years before, three years before and four or more years) as well as province-year dummy variables controlling for province-wide macroeconomic effects. The cluster corrected *F*-Statistic on the full set of 23 dummy variables is significant, at 2.17, with a *p*-value of 0.002. In column 1, the *F*-statistic testing the joint hypothesis that coefficients on all "pre-treatment" dummy variables are zero is 0.48, and none of the coefficient estimates on the individual variables are statistically significant, suggesting that the timing of ID distribution is not simply associated with migration behavior before ID distribution. Excluding the pre-treatment dummies does not lead to a higher *F*-statistic on the 19 post-distribution dummy variables (column 2), and the low F-statistic argues against using a full set of dummy variables to identify the village migrant share.

We next turn to alternative specifications for the relationship between timing of ID distribution and migration (Table 3). Recognizing from estimates in Table 2 that the effect of ID availability takes a few years to build up, a first alternative uses a single dummy variable indicating that cards were issued more than three years in the past. The cluster corrected *F* -

[3]Government of the People's Republic of China. 2005. Administrative Divisions of the People's Republic of China (中华人民共和国行政区划; Zhōnghuá Rénmín Gònghéguó Xíngzhèng Qūhuà). June 15, 2005. http://www.gov.cn/test/2005-06/15/content_18253.htm.

statistic on the single indicator variable is 7.6. Column (2) shows six dummies for years since distribution grouped for 1-2, 3-4, 5-6, 7-8, 9-10 and 11+ years, which are jointly significant but with an F-statistic of 3.4. In column (3) we show the coefficients on a quadratic in years-since-IDs were issued, and a quartic in column (4).

Figure 5 shows a plot of the predicted migrant share variable against years-since-IDs were issued using both coefficients on the dummy variables (Table 2, column 2) and the quadratic and quartic plotted from coefficients from the specifications shown in columns (3) and (4) of Table 3. Note that the quartic does a particularly good job of mimicking the pattern of the predicted coefficients on the full set of dummy variables. While the cluster-corrected F -statistic on the quartic is low at 3.66 and lower than that for the dummy variable for 3 years since cards were issued, we prefer the quartic as it better measures the build-up of the migrant network over time. In our estimation below, we further take care to evaluate the possibility that our results are affected by weak instrument bias.

To more directly assess whether the years-since-IDs instruments might be associated with high school enrollment trends, columns (3) and (4) of Table 2 show the reduced form effect of ID distribution. Both pre-treatment and post-treatment years are included in Column (3), and coefficients on dummy variables for years before IDs were issued are not jointly significant. The lack of a pre-distribution effect again suggests that the instruments based on years-since-IDs were issued are not simply measuring long-term trends. In column (4) of Table 2, we show the non-parametric reduced form using only post-treatment dummy variables. After four or more years from distribution, these dummy variables generally carry negative coefficients that are individually significant.[4]

Another issue, briefly flagged above, turns on our assumption that children started school at age seven, the modal starting age evident in other surveys, and were unlikely to skip or repeat when progressing through grades. In practice, and in all villages, we know it likely that some students began school at age 6 and some at age 8, and a small percentage likely skipped or were held back. How might assumption of an age 7 start affect our estimates? First, it is unlikely that our preferred instrument, based on years-since-ids became available, is systematically associated

[4]Nonetheless, when later discussing the results of migrant opportunity on education, we check that results are robust to including village trends in educational attainment.

with unobserved differences in child ability. Second, we know whether the decision to enroll in high school was made, but being off by a year will imply that for some observations the predicted increase in migrant share from the village will not be perfectly mapped into the year of the decision. The quartic in years-since-IDs is a smooth function, and we do not observe extremely sharp jumps in the predicted migrant share variable from one year to the next, and so we believe it unlikely to be systematically related to within village variation in unobserved ability.

While we may not worry about small errors in assumed date of school entry, or lack of a pre-treatment trend for ID distribution, these observations do not rule out the possibility that ID distribution was driven by increases in the number of local residents wanting to migrate, which may have arisen either in the same year or in the year or two preceding ID distribution. As earlier work finds that large prior year rainfall shocks are associated with out-migration (Giles and Yoo 2007), we address this possibility by estimating hazard models for ID distribution. In these models, we examine the possible links between ID distribution and exogenous rainfall shocks, which affect local agricultural productivity and therefore the returns to labor in both agricultural and non-agricultural sectors. When large negative shocks occur, incentives to migrate increase and families may pressure village or county leaders to distribute IDs.

We construct our preferred measure of "significant rainfall shocks" from thirty years of monthly rainfall data collected from county weather stations in each of the 52 counties where RCRE villages are located. The shock we use is the square of the deviation of July-November rainfall from the thirty year average rainfall over these months. The dependent variable in the estimated hazard models takes a zero before IDs are issued and one in the year of issue, after which the village drops from the dataset. The indicator for ID distribution is then regressed on province-year dummy variables and the squared rainfall shocks. In successive models shown in Table 4, we use the one year lag in the squared rainfall shock, the two year lag, and the combination of the two lags to assess whether negative shocks to the local economy might affect the demand for IDs and influence the timing of their distribution. Not only do we find no significant relationship between rainfall shocks and the timing of ID distribution, but point estimates of the marginal effects are also quite small. The combination of these factors lend confidence that incentives to migrate did not drive the timing of ID distribution.

The timing of ID distribution may not be correlated with trends in educational attainment or with shocks to the local economy, but may yet be systematically related to other features of the local economy or village policy. As some minor differences in village characteristics are apparent in 1988, the modal year of ID distribution, we control for fixed village unobservables with a village fixed effect.

Apart from fixed unobservables, like geographic proximity to cities, one might remain concerned that the timing of ID distribution is systematically related to other time-varying village level policies or administrative capabilities. In turn, these policies may affect both migration and high school enrollment decisions. For example, village leaders have considerable control over implementation of grain procurement policy and land use by village residents, and so it is of interest to know whether decisions regarding these policies are systematically related to the timing of ID distribution. If a systematic relationship exists, the instruments could proxy for factors other than migrant opportunity that influence the high school enrollment decision.

One might imagine estimating logit hazards as in our test of whether rainfall shocks influence ID distribution, but such an approach has less appeal because we lack 30 years of village data to construct proxies for policies, and further, because policy changes may be endogenous. We thus look for direct evidence of whether our instrument is correlated with proxies for time-varying village policies and administrative capacity. These proxies, VP_{jt}, are each regressed on the quartic in years-since-IDs, village fixed effects, province-year effects, and household and village characteristics:

$$(3) \qquad VP_{jt} = \gamma_j + \alpha_1 ID_{jt} + \alpha_2 ID_{jt}^2 + \alpha_3 ID_{jt}^3 + \alpha_4 ID_{jt}^4 + \boldsymbol{Z'}_{jt}\boldsymbol{\theta}_2 + \boldsymbol{X'}_{hjt}\boldsymbol{\theta}_3 + \boldsymbol{p} \cdot \boldsymbol{T_t} + \nu_{\boldsymbol{jt}}$$

In Table 5, we report F -tests on the quartic in years-since IDs were issued in specifications that both exclude (column 1) and include the vectors of household and village level characteristics (column 2). Initially, we examine local implementation of the grain quota policy. The grain quota, which was phased out between 2001 and 2004, was effectively a tax in which farm households were forced to provide some grain to the government at below market price. The quota also constrained the decisions of households in those villages in which households are unable to provide cash or purchased grain in lieu of grain that the family had produced. When the quota share of grain produced is closer to one, quota policy is more likely to be driving the decision to continue growing grain crops. In rows 1 and 2, we observe that the quota share of grain produced has no systematic relationship with the years-since-IDs were issued.

We next test whether three indicators related to village land tenure security and land use are related to ID distribution. While farmers nominally had fifteen and then thirty year leases on their land over the 1986 to 2003 period, the leases were treated as policy and it was not uncommon for village leaders to reallocate land much more frequently (Brandt, Rozelle and Turner 2004). The share of land in the village planted in orchard crops, and the share of households renting land in and out are all proxies for household perceptions of long-term land tenure security. Throughout the survey region, orchard crops are typically of higher value and far more labor-intensive in production than land-intensive grain crops. Planting orchard crops, however, requires a specific long-term investment that households may be unwilling to make when land tenure is insecure. Further, the transfer of land through rental arrangements will not occur in areas where a rental transaction is taken as a signal that a household no longer needs its land, and may thus lead to subsequent expropriation, or where villages place excessive administrative procedures and conditions on rental transfers. We observe no statistically significant relationship between share of land cultivated in orchard crops or share of households engaged in rental contracts and the timing of ID distribution in the counties where villages are located.

Finally, we examine the relationship between the weighted average local tax rate paid by households and timing of ID distribution (Table 5, row 6). During the study period, villages charged a range of different administrative fees to support investment in local public goods and to cover any village administrative costs. The weighted average village tax rate is a useful indicator of the administrative capacity of the village. If village administrative capacity is related to timing of ID distribution, because more capable village leaders are better at lobbying higher levels of government for IDs, then this capacity may also affect motives for migration and high school enrollment. We find no significant relationship between the time-varying weighted average village tax rate and the timing of ID distribution.

4. Results

A. Effects of Migration in the Basic Model: Test Statistics on Weak Instruments

As non-linear discrete choice models controlling for endogeneity require restrictive assumptions on the error term, we estimate equation (2) using the linear probability model. Further, as noted above, and based on results in Tables 2 and 3, we must carefully consider the possibility of weak instrument bias. We thus first estimate equation (2) using different transformations of the years-since-IDs instrument and show results for the coefficient on village migrant share for different specifications, initially including only village fixed effects and province-year fixed effects as additional covariates (Table 6). We show results for the fully non-parametric specification in column (1), a dummy variable for more than three years since distribution in column (2), dummy variables grouped in two year increments in column (3), a quadratic in years-since-IDs in column (4), and the quartic in years-since-IDs in column (5). The first stage estimates can be found in column (2) of Table 2 for the first specification and columns (1) through (4) of Table 3 for the second through fifth specifications. We separately estimate each specification with an IV estimator, an IV-GMM estimator and a Limited Information Maximum-Likelihood (LIML) estimator, which is generally less biased under weak instruments (Wooldridge 2002; Baum, Schaffer and Stillman 2003).

Examining results for each instrument set using different estimators, note that point estimates using the standard IV procedure and the IV-GMM procedure are nearly identical. As the test statistics are the same for IV with clustered standard errors and IV-GMM, we present them once for the two IV estimators and once for the LIML estimates (e.g., Stock and Yogo 2005). Cameron and Miller (2011, 2015) emphasize that the "rule of thumb" for the presence of weak instruments is based on F-statistic thresholds derived using i.i.d. error terms, yet appropriate *F*-statistic thresholds for the presence of weak instrument bias are likely to vary by application with cluster corrected *F*-statistics. For our primary test statistic, we thus follow Cameron and Miller and implement a cluster robust version of Moreira's (2003) Conditional Likelihood Ratio test, derived by method of moments (Finlay and Magnusson 2009). We use this test statistic to generate weak instrument robust 95 percent confidence intervals around the coefficient estimate on the migrant share variable.

Reviewing the results, recall that the fully non-parametric model has a relatively low clustered F -statistic of 2.17. By contrast, using only a dummy variable for more than three years since IDs were distributed leads to a much larger clustered F -statistic of 7.64 and a coefficient estimate on migrant share of −3.12, significant at the 10 percent level. Examining other sets of instruments, the grouped dummies, the quadratic in years-since-IDs and quartic in years-since-IDs yield coefficients on the village migrant share variable in the LIML models of −1.55, −4.09 and −2.62, respectively, with clustered F -statistics of 3.4, 4.1 and 3.6. These coefficients are all statistically significant at the 5 percent level or better, and nearly identical in the IV and IV-GMM models. Moreover, the estimated coefficients on the village migrant share in models using the quadratic and quartic in years-since IDs are not significantly different from the model using the dummy variable for more than 3 years since ID distribution. The coefficients on migrant share are more precisely estimated using polynomials in years-since-IDs, reflecting the greater specification flexibility afforded by these models. The *F*-statistics on the first stage for our preferred models are suggestive of weak instruments, but the CLR test demonstrates that the coefficients on village migrant share in over-identified models are negative and statistically different from zero for the specifications using groups of dummy variables, the quadratic and quartic in years-since-IDs became available.

Are the coefficients on the village migrant share reasonable? Until one considers the demographic structure of the migrant population, who were primarily young, the magnitude of the effect on high school enrollment implied by the coefficient on village migrant share may seem improbably large. Recall that much of the share of the working age population of a village employed as migrants is under 30. Consequently, a one percentage point increase in the migrant share of the village workforce would be concentrated among younger cohorts. We use the age structures of the migrant population and the village working age population, both shown in Appendix Figure A.7, to project how an increase in the migrant share of the village workforce would affect the migrant share of five year age cohorts (Figure 6). In 2001, a one percentage point increase in the village migrant share is associated with a four percentage point increase in the percentage of 16-20 year olds working as migrants. Once we account for the age structure of migrants as a component of the village workforce, the coefficient on village migrant share in the high school enrollment model seems quite plausible.

B. Main Results: The Effect of Migration on High School Enrollment

We continue our investigation of the relationship between migrant opportunity and high school enrollment by backtracking and estimating equation (2) using OLS, controlling for province-year and village fixed effects (Table 7, column 0). The estimated coefficient on the size of the village migrant labor force is −0.097, and it is not statistically different than zero. Without controlling for the endogeneity of migration, there appears to be no relationship between high school enrollment and migration. As noted above, however, factors such as expanded capacity in high schools or a decline in the cost of attending high schools through improved roads and public transportation may well be endogenous with factors that simultaneously lower the cost of participating in the migrant labor market.

We next add individual, household, and village level covariates, summarized in Table 8 and with additional detail in Appendix Table A.5. As covariates are added to equation (2), we find the share of migrants in the village workforce consistently has a negative, statistically significant effect on high school enrollment (Table 7, columns 1 through 5).[5] We first reproduce the IV-GMM results shown in column (5) of Table 6, in which we include only province-year and village fixed effects, and show that a 1 percentage point increase in the village migrant share is associated with a 2.4 percentage point decrease in the probability a middle school graduate will enroll in high school the following year. As the migrant share of the village labor force increases, and presumably the size and quality of the network improves, the net return to migrating and the opportunity cost of staying in school rises enough that we observe a substantial decline in probability of high school enrollment.

To account for time varying village economic conditions, we initially add time-varying village controls to equation (2) (Table 7, column 2). While none of the village level coefficients differ significantly from zero, the inclusion of these variables is associated with an increase in the first stage F -statistic on the instruments to 4.4. Importantly, the inclusion of these variables does not change the point estimate of the effect of migration on high school enrollment.

Individual and parental characteristics may also affect the decision to attend high school by contributing to differences in levels of household wealth or family preferences for education. We initially add gender, an indicator variable for whether the child is first born, and an indicator variable for households in which the first born child was male (Table 7, column 3). From these coefficient estimates, it is apparent that girls with older male siblings will be less likely to attend high school. The gender preference holds even after including additional family characteristics, but the positive effects of being first-born become less important and insignificant.

We next add parental characteristics that reflect innate ability, proxy for wealth, and the ability to migrate, and continue to find that migrant opportunities negatively affect high school enrollment (Table 7, columns 4 and 5). Both the father's and mother's years of schooling positively affect an individual's likelihood of attending high school (column 4). The number of

[5]First stage regressions for Table 7 can be found in Appendix Table A.7.

potential migrants from the household has a negative, statistically significant effect on high school enrollment (column 5). Thus, when more current and former household members are of an age to migrate and households have more information about employment opportunities, children are less likely to attend high school.

Other time-varying village characteristics reflecting market development could affect the relationship between migrant networks and high school enrollment decisions. For example, if IDs facilitate trade between local firms and distant partners, or make it easier for families to claim social benefits (e.g. health insurance or enrolling children in school), then issuing IDs may affect other activities that also influence migration. To account for characteristics related to market development and the local impact of state intervention, we include another vector of time varying village level variables in our model (column 6). The vector includes the average share of grain sold at quota prices; three variables measuring land use in activities other than grain or legume production; and average household wealth per capita in the village and the proportion of households with some non-agricultural self-employment (both excluding household *i*). We jointly test the null hypothesis that estimated coefficients on these variables are zero, and cannot reject the null. More importantly, their inclusion does not affect the estimated coefficient on M_{jt}. As a result, it is unlikely that time-varying unobservables related to market development bias our results. In our remaining analysis, we exclude this vector as it only introduces noise into our regressions, and further specifications in the paper include the variables in column (5) of Table 7.

The additional set of time-varying village controls included in column (6) do not control for the possibility that the years-since-IDs instrument simply measures village specific trends in educational attainment. To test whether we pick up such trends in lieu of an actual relationship between migration and high school enrollment, we isolate village specific pre-treatment trends in education by regressing educational attainment among those over age 20 in 1984 on age dummies separately for each village to obtain a fitted value for changing educational attainment within the village over time. We then interact the village specific fitted trend with survey year fixed effects. When these village-specific education trends are included in the regression, the point estimate of the coefficient on migrant share remains negative, but is no longer statistically significant. As we can accept the null hypothesis that the coefficients on the trend variables are jointly zero, we conclude that the pre-treatment trend does not appear to be important for explaining current enrollment decisions.

In sum, when controlling for village, household, and individual characteristics, the estimated effect of village participation in migrant labor markets remains negative. In each case, the coefficient estimate is between −1.80 and −2.43 and significant at the 5 percent level. In all specifications, over-identification tests on the quartic in years-since-IDs offer further support for the hypothesis that the instruments are not systematically related to unobservables influencing both migrant opportunity and high school enrollment.

To provide a further robustness test, we test an alternative IV approach in Appendix A.2, which makes use of time-series information on city level GDP collected by China's NBS to proxy for annual changes in labor demand in potential migrant destinations. The instrument is a function of GDP growth in the nearest city outside the home county and the distance from the village to that city. When this instrument is used in a basic version of equation (2), regressing high school enrollment on the village migrant share, village fixed effects and province-year effects, the coefficient on migrant share is −2.12, while it is −2.43 in the model with the quartic in years-since-IDs was distributed, and the difference in these coefficient estimates is not statistically significant.

Given that our primary interest lies in understanding how migration affects incentives to enroll in high school, we have devoted our attention to the coefficient on the migrant share of the village labor force in the IV-GMM model. To understand the performance of the instruments, the reduced form effect of introducing IDs on the probability of enrolling in high school is also of interest. We thus also estimate the reduced form:

$$(4) \qquad E_{ihjt} = \alpha_0 + \alpha_1 ID_{jt} + \alpha_2 ID_{jt}^2 + \alpha_3 ID_{jt}^3 + \alpha_4 ID_{jt}^4 + \boldsymbol{Z'}_{jt}\boldsymbol{\alpha}_5 + \boldsymbol{X'}_{hjt}\boldsymbol{\alpha}_6 + \boldsymbol{u}_j + \boldsymbol{p}\cdot\boldsymbol{T}_t + e_{ihjt}$$

where *ID* is the years-since-IDs were issued in the county where the village is located. The estimates $\hat{\alpha}_1$, $\hat{\alpha}_2$, $\hat{\alpha}_3$, and $\hat{\alpha}_4$ can be used to predict the effect of ID availability on the probability of enrolling in high school after controlling for province-wide trends in enrollment, time-varying household and village characteristics and village fixed effects. In the pure reduced form, the predicted effect of ID availability on the probability of enrolling in high school is pronounced and negative (as illustrated in Panel A), while the effect on migrant share is positive. This negative probability is a deviation from a positive trend toward increased enrollment. In Panel B, we illustrate a trend based on average predictions from a regression including only village and province-year effects and plot across calendar years, instead of years-since-IDs became available. Next, we add back the quartic in years-since-IDs to illustrate the deviation from trend, and finally, we show the actual average trend in the data. This illustration shows that the increase migrant opportunity slowed the rate of increase in high school enrollment that was otherwise occurring.[6]

C. Heterogeneity in Preferences for High School Enrollment

Families with different characteristics will not necessarily respond to migrant opportunity in the same manner. Even within villages, families with differences in educational preferences, information about employment opportunities, and credit constraints affecting the ability to attend school are likely to make different high school enrollment decisions. By examining these dimensions of heterogeneity, we may better determine the relative extent to which the opportunity cost of attending high school drives the enrollment decision. Furthermore, by examining heterogeneity in enrollment patterns we can address a potential concern that estimates in Table 7 are an artifact of convergence in access to, or the cost of attending, high school across more and less remote communities.[7]

To begin to examine heterogeneity in preferences for high school enrollment, we first expand equation (2) to include indicator variables for the following characteristics of the child's father in the year prior to the high school enrollment decision: *father is a "professional"* (party member, village leader or enterprise manager); *father ever had off-farm employment* (in either the migrant or local wage labor market); *father ever enrolled in high school*; and *father completed high school*. If a child completing middle school has a father with professional status, he or she is 13.7 percent more likely to enroll in high school (Table 9; column 1, row 2). A child with a parent

[6]A last potential concern with using the quartic in years-since-IDs were issued as the instrument is that the quartic could be sensitive to outlier villages. With this concern in mind, we successively re-estimated the model restricting the sample by dropping the three earliest ID adopters at the village level, the three latest adopters, the three highest and lowest initial migration villages, and the three villages with the most pronounced growth in out-migration. In none of these models did we find qualitatively significant differences in the econometric results.

[7]For example, parents with children of low ability, or who have low preferences for education, may keep their children out of high school regardless of whether a school is available near their community. With construction of new schools in remote regions, high ability students who were not previously able to attend high school will now do so. One might be concerned that the years-since-IDs instrument is picking up convergence across communities, and thus timing of distribution is associated with the timing of when parents have the ability to decide whether or not to send a child to school, and not the the effects of migrant opportunity.

who is a cadre or enterprise manager is less likely to be credit constrained, and the parent is likely to have high ability and be better situated to influence high school admissions decisions. Similarly, a child whose father ever enrolled in high school is 16.9 percent more likely to enroll in high school (column 1, row 6). Children with parents who have some high school education will also be in families that are likely to have higher income and preferences for more education.

A lower skilled family will face fewer constraints to enrolling a child in high school if a father has off-farm employment experience. This is because off-farm employment is associated with higher income in most analyses of household income in rural China, including those using this data sources (e.g. Benjamin, Brandt and Giles 2005). Nonetheless, we observe a negative but statistically insignificant coefficient on *father ever had off-farm employment*. Parents with off-farm employment experience not only have higher earnings, but also more familiarity with the off-farm employment opportunities for middle school graduates.[8]

We examine the heterogeneous impact of migrant opportunity on the high school enrollment decision by allowing the effect of village out-migration to vary with father occupational and educational background. We interact the migrant share of the village workforce with the four indicator variables for father characteristics in columns (2) through (5). Each interaction term is treated as endogenous, and interactions of the dummies with the quartic in years-since-IDs are used as instruments. As the migrant share of the village labor force increases, the probability that a middle school graduate with a professional father will enroll in high school falls significantly (Table 13, column 2). At the mean level of migration in the sample, the predicted effect of father being a professional on high school enrollment drops to 10.2 percent, or 30 percent lower than the estimated effect shown in column (1). If one had been concerned that the earlier estimates reflected the enrollment decisions of individuals with less ability to attend high school, this estimate is quite important, as it implies the effect of migrant opportunity is actually stronger on the high school enrollment decision among children who were otherwise more likely to attend high school.

We find a similar result when examining the effect of migrant opportunity on children with a father working in migrant or local wage employment in the previous year. Since households that

[8]We use the "father is professional" and "father ever had off-farm employment" variables to proxy for household wealth because including more direct wealth measures would risk introducing endogeneity bias.

participate in off-farm wage labor markets have higher incomes, one might expect that children from such families would be more likely to attend high school as credit constraints are relaxed. Instead, we find that middle school graduates are less likely to enroll in high school if their fathers worked off-farm than children with fathers who did not work off-farm (column 3). Similarly, increases in migrant opportunity are also associated with lower probability that children with more educated fathers enroll in high school (column 4). Thus, even children from families who may have parents with strong preferences for more education are influenced by the perceived opportunity costs of remaining in school.

D. Potential Issues Related to Exclusion Restrictions

Another concern regarding the years-since-IDs instruments is that they may pick up other effects that directly or indirectly influence the high school enrollment decision. Families with migrants already in the city might already experience a positive wealth effect, which could influence the decision to enroll in high school. If the wealth effect eases household credit constraints, it would lead to an upward bias in high school enrollment. However, it may have other effects on career choice other than simply reducing the cost of finding work. With potential biases from this source in mind, we re-estimate our main models (in Table 7) excluding individuals from households who had migrants at the time of ID distribution. Only five percent of sample households fit this category, and not surprisingly the results do not qualitatively differ when these households are removed from the sample.[9]

Another potential pathway affecting high school enrollment might be related to the impact of parent migration on child well-being and child effort in elementary and middle school. For example, Chen (2013) finds that children spend more time in household production when fathers migrate. While this is a matter of current policy concern, and scholars working with data from later in the 2000s have identified this potential channel, few middle school graduates making decisions to enter high school over the period we are studying had parents with prior migration experience (Figure 8). Between 2000 and 2003, fewer than 5 percent of children aged 13 to 15 had lived without a parent for some part of their lives.

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[9] Using the quartic in years-since-IDs were issued as instruments, we found the estimated coefficient on the village migrant share was −2.27, with a standard error of 1.11 in models including only village dummy variables and province-year dummies, and -1.83 with a standard error of 1.01 when we include all control variables in column 5 of Table 7.

E. Evidence on Returns to High School in Urban Areas

Low marginal returns to high school education, relative to middle school and university, is consistent with segmentation of the urban labor market between urban and rural *hukou* holders (Meng and Zhang 2001), and further supported by Li et al. (2012), who use twins data from 2002-3 to show that returns to high school education in urban China were close to zero at this time, while returns to a year of academic and vocational technical tertiary education are 10 and 5 percent, respectively. In Appendix A.3, we use a Heckman selection model and data from the 2003 RCRE survey round to present supporting evidence of the low return to high school education for migrants in our sample. For the 2003 survey round, questions regarding the daily earnings of migrants were included in the regular annual survey, allowing us to examine returns for different levels of schooling. We introduce a piecewise linear spline for years of schooling between 0 and 9, 10 to 12 years and more than 12 years. Consistent with other work examining the returns to education in urban areas over this period, one observes positive and significant returns to education for individuals with less than nine years of education, returns near zero for migrants with 9 to 12 years, and positive but insignificant returns for migrants with educational attainment above 12 years.[10]

F. Direct Evidence on the Activities of High School Age Children

With the growth of migrant networks from the village, the local labor market may also experience general equilibrium effects that increase the opportunity cost of high school enrollment. As migrants leave the village, the local labor force decreases in size, which in turn may increase the return to labor in home production (agricultural or non-agricultural family

[10]As we use average daily wages reported by household members on behalf of the migrant worker, one might expect that these are low estimates because migrants were unlikely to have been working for the full number of days that they were outside the household for work. Further, unlike the follow-up survey which asks about all children of the household head and spouse, the 2003 survey on migrant incomes only asks about earnings of migrants still registered as household members. Because migrants with more than secondary education are more likely to convert to urban *hukou*, there are relatively few observations with more than secondary education and point estimates of returns are well below what we would expect for this level of educational attainment. The main analysis sample, based on the follow-up supplemental survey conducted in 2004, includes information on the educational attainment of all current and former household residents, but did not enumerate estimates of current or past earnings of these individuals.

businesses) or local off-farm wage employment sufficiently to dissuade teenagers from attending high school. While we lack individual information on daily earnings over time, we investigate the effects of village migration on the following-year activity choices of individuals in the two years after they would normally have completed middle school. We expect that as the size of the migrant network increases, the local labor force is depleted and, as a result, we may observe that teenagers are more likely to participate in local labor market activities. Moreover, during the period covered by this study, residents could not apply for a national ID until after their 16th birthday, and processing times for the ID could be six months or longer, particularly before 2000. Apart from institutional barriers preventing teenagers from moving to migrant jobs upon completing middle school, parents may also wish their children to delay migration to their later teens and to gain experience in local employment first. Thus, even if the return to costly investments in high school are quite low for would be migrants, it might not be surprising to find teenagers engaged in activities other than migrant work.

To examine how migrant network size affects activity choice, we use activity choice indicator variables contemporaneously with, one year since, and two years since the high school enrollment decision (Table 10). We again use the linear probability model and IV-GMM, and report the estimated coefficients on the village migrant network share. When we examine the next year's activity choice for children that would normally complete middle school in the current year, we find that results are not strong for any activity (row 1). This result may be driven either by measurement error in estimates of when individuals completed school and when they began their first work activity or by a considerable waiting period after middle school completion. Results for next year's primary activity choice for individuals of an age to complete middle school one or two years earlier (shown in rows 2 and 3) suggest that 16 and 17 year olds are more likely to participate in either the local or migrant wage labor market. Again, institutional features related to acquisition of IDs after the 16th birthday may encourage off-farm local employment first. Further, any general equilibrium effects created by a declining local labor force with out-migration may reinforce the negative effect of potential employment in migrant destinations on the perceived costs of remaining in school. The negative coefficients on migrant share for models examining work in agriculture or household self-employment suggest that young people are not substituting for parents in local household activities. This finding rules out the possibility that migration leads to exit from high school because teenage labor within the household substitutes for that of other household members who have migrated.

5. Conclusions and Discussion

The movement of rural laborers out of agriculture into urban and coastal areas has been an important feature of China's economic transition. While the opportunity to migrate has raised living standards in many areas of rural China, access to migrant employment appears to create a disincentive for rural youth to enroll in high school. This finding may be important for two reasons: first, rural children may not recognize the real return to education, and second, there may be negative externalities associated with growing inequality between Chinese residents of urban and rural origins.

Much of the information received by rural children and their families regarding the returns to education flows through networks of migrants with middle school education. While the real private return to high school may be higher as it is an input into post-secondary education (Appleton Hoddinott and Knight 1996), this return may not be observed or may be viewed as unattainable by potential migrants who do not expect to enter university. College tuition rose nearly 600 percent between 1996 and 2001 (Du and Giles 2006) and student loans and scholarships were only available at a few national key universities, thus placing college attendance outside the reach of most rural families. From the census, for example, it is evident that school enrollment of urban men at age 20 rose from 33 percent in 1990 to 58 percent by 2005, while enrollment of men with rural registration increased from roughly 5 to 10 percent over the same period (Appendix Figure A.6). While the increase in rural areas is large in percentage terms, it suggests that the prospects of enrolling in college and realizing the returns to investments in high school are remote. Expanding access to student loans and scholarship support for students outside of key universities may ease credit constraints, and providing rural families information about both the feasibility of enrolling in university and the returns to post-secondary careers, may encourage more rural children to continue on to high school.

Apart from the possibility that rural residents do not recognize the full return to high school, China's policy makers remain concerned about possible negative externalities created by increasing inequality, which is reflected within urban areas increasing gaps in the living standards between residents of urban and rural origins (World Bank and Development Research Center 2012). Policy-oriented researchers have also recently expressed fears that rural born young adults who were drawn into construction jobs in the wake of China's recent stimulus-fueled infrastructure construction boom, and who typically sought lucrative work after completing middle school education, may find it difficult to acquire new skills as construction slows (Du and Cai 2013). Reducing the permanent component of inequality within urban areas and improving the ability of future migrants to shift to more skill-intensive occupations may both be facilitated by reducing the costs of high school education for rural students capable of continuing their education beyond middle school.

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Figure 1
Share of Cohort Entering High School by Gender
Lowess Fit

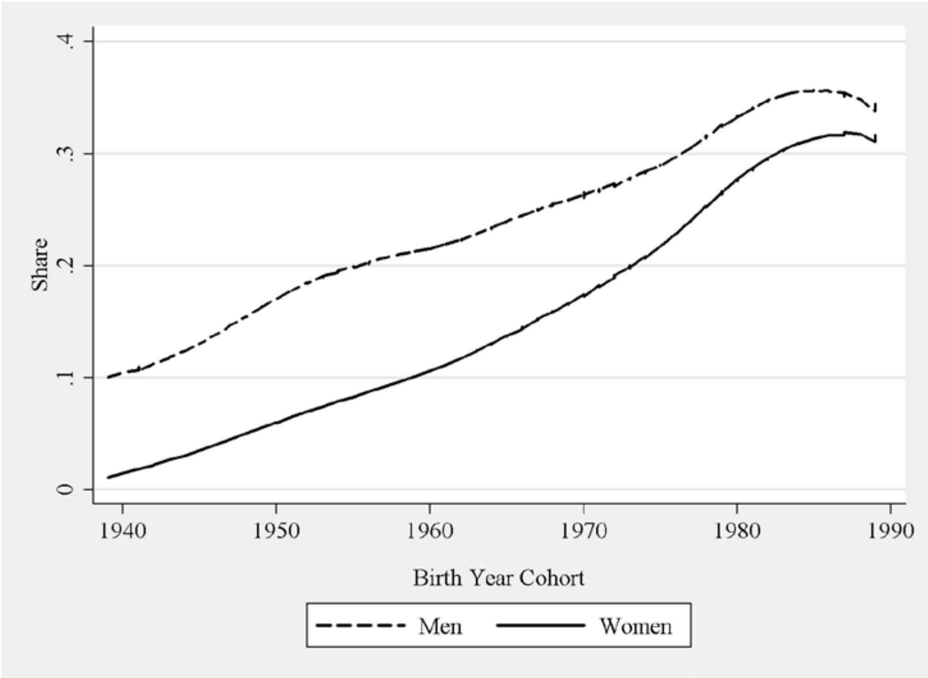
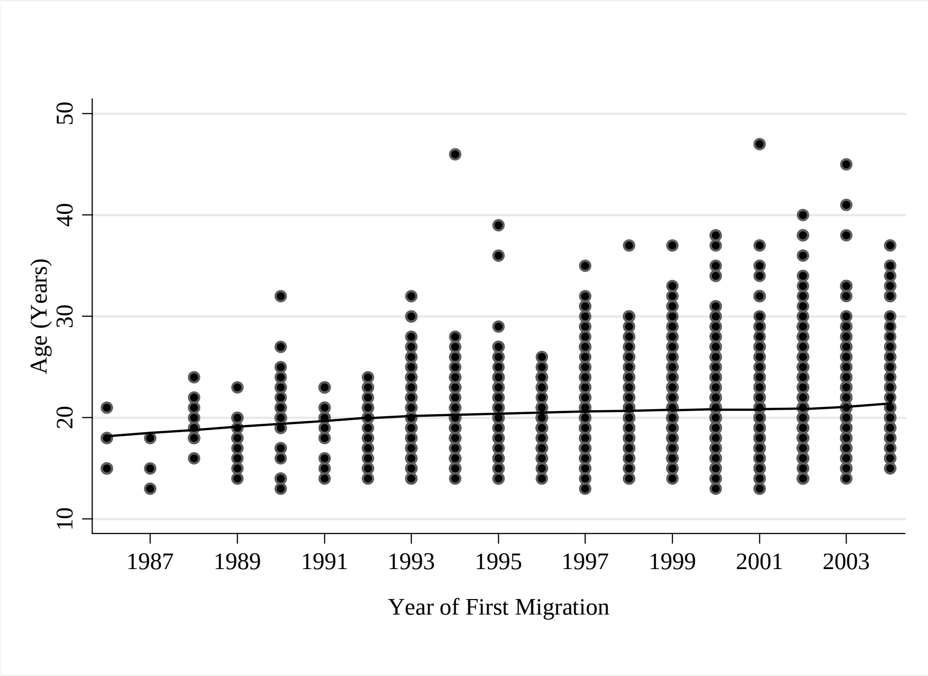
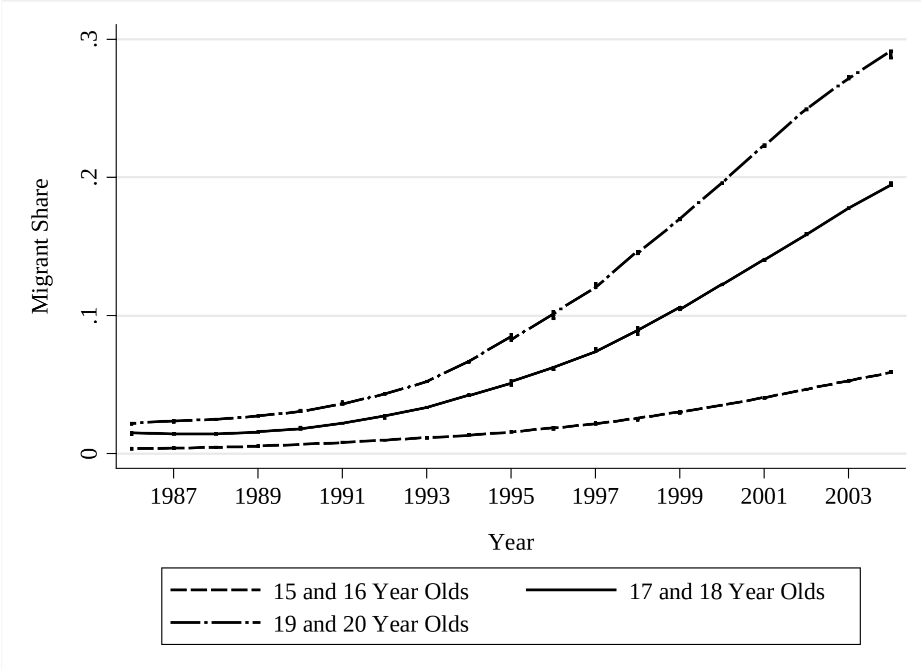


Figure 2
Age at Time of First Migration Experience
Individuals Growing Up in RCRE Villages



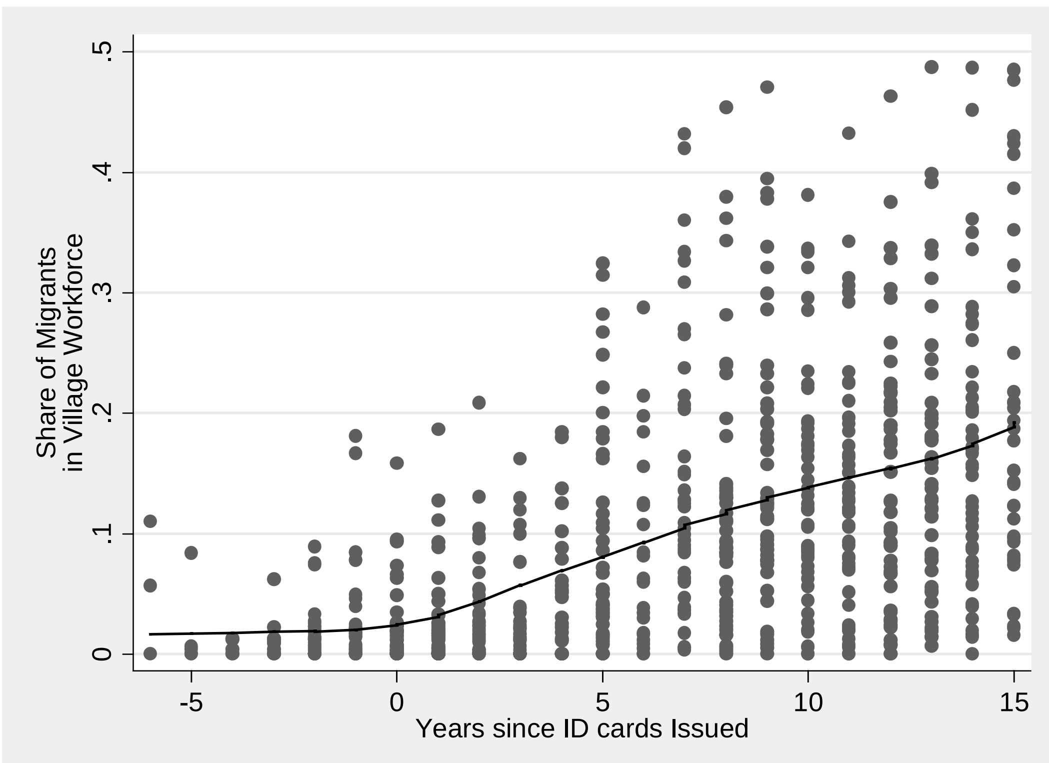
Source: RCRE Supplemental Survey (2004).

Figure 3
Share of Age Group with Temporary or Long-Term Migrant Employment
Individuals Growing Up in RCRE Villages



Source: RCRE Supplemental Survey (2004).

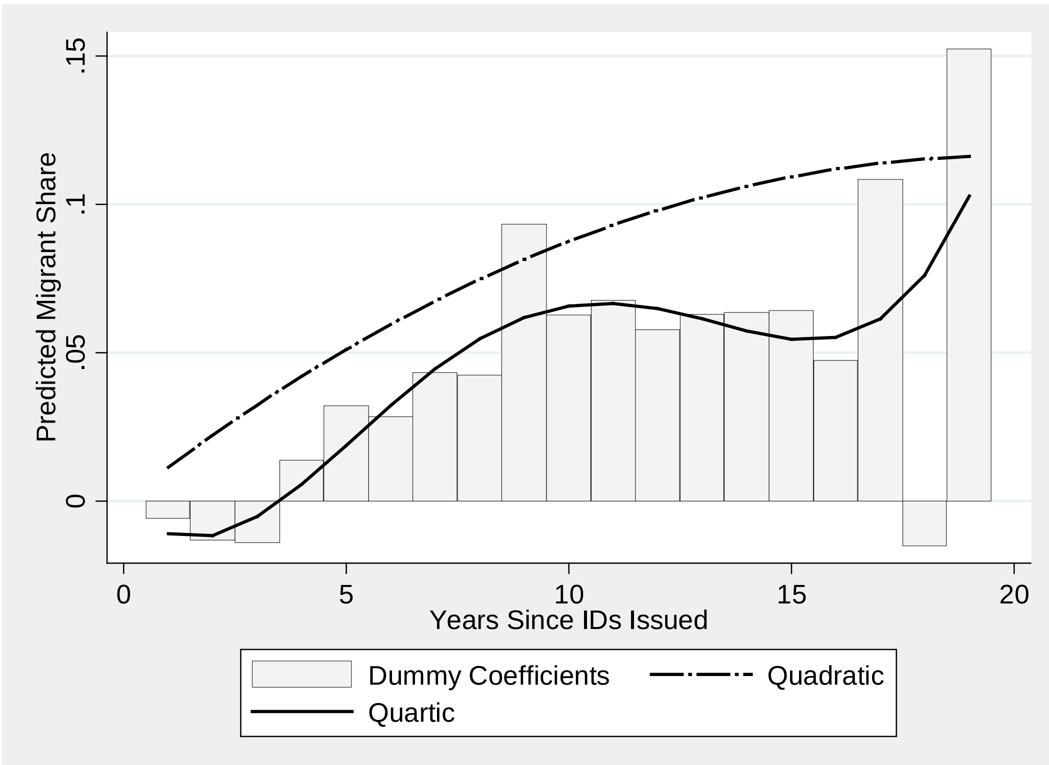
Figure 4
Share of Migrants in the Village Workforce, by Years since ID cards were Issued



Source: RCRE Supplemental Survey (2004).

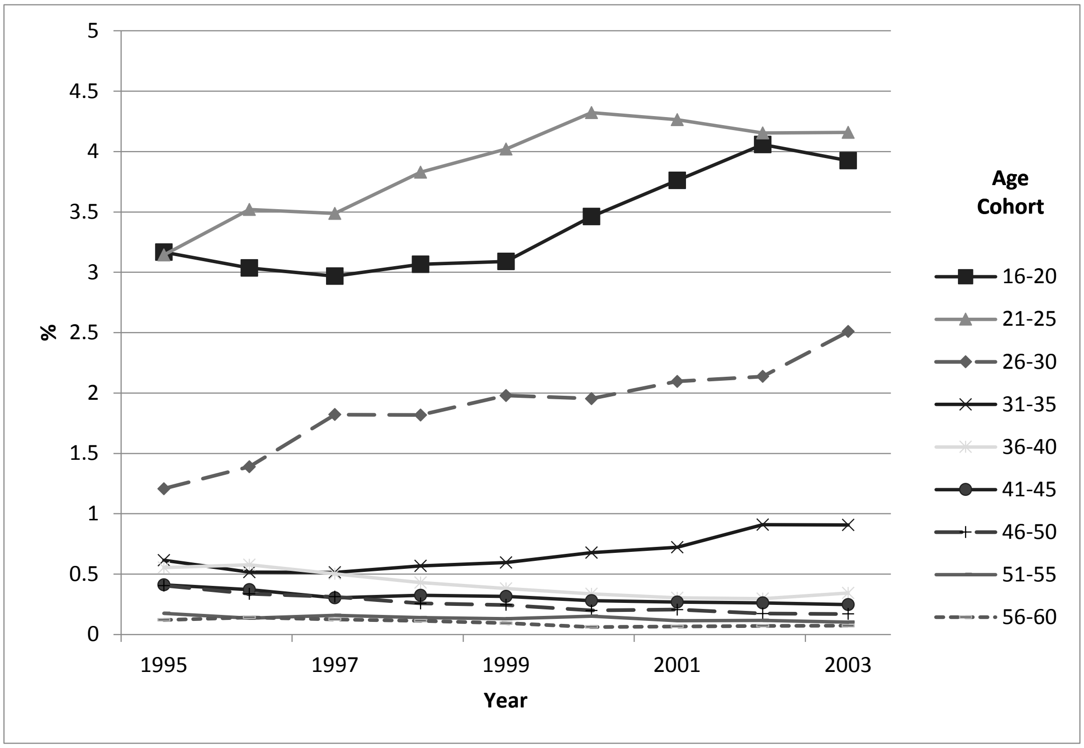
Figure 5

Estimates of Migrant Share Against Years Since IDs Issued, using Dummy Variable, Quadratic, and Quartic Specifications



Source: RCRE panel (1986-2003) and Supplemental Survey (2004).

Figure 6
Projected Increase in Share of Age Cohort Working as Migrants after a One Percent Increase in Village Migrant Share



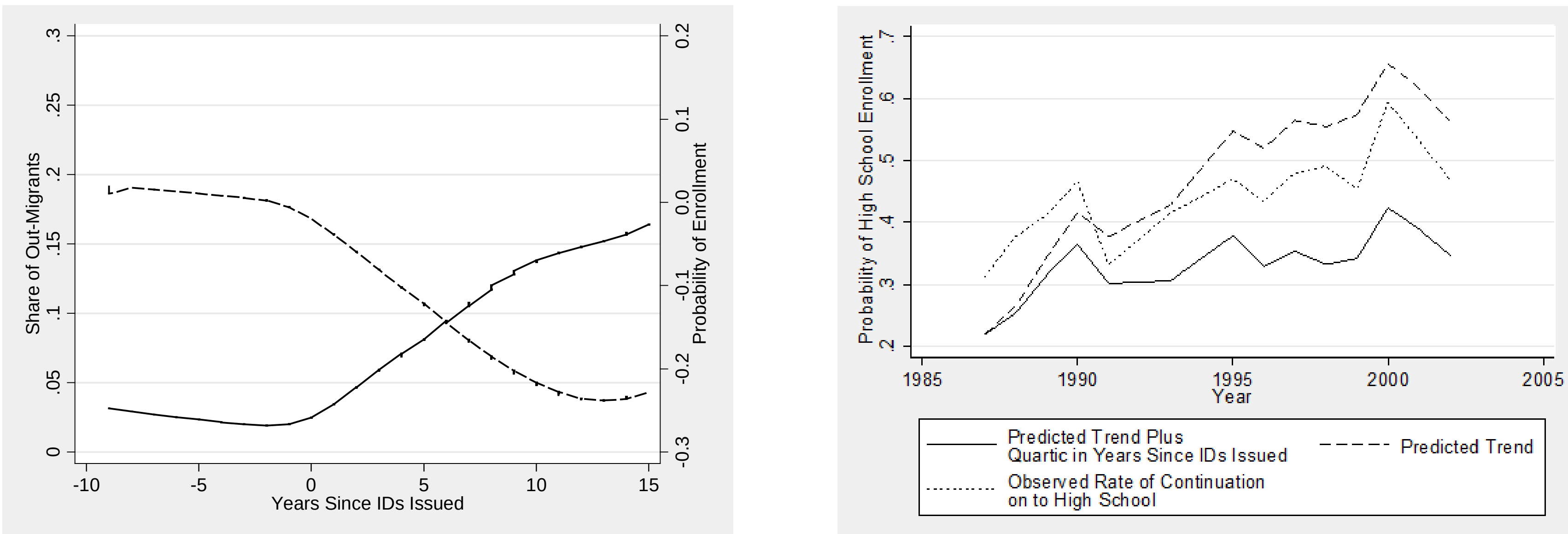
Note: This graph calculates the percent increase in migration by age cohort corresponding to a one percent increase in the share of the village workforce working as migrants. We do so by computing how many additional migrants would be implied by a one percent increase in each year *t*, and multiply by the share of migrants in each age cohort in the sample to understand how the one percent increase affects each cohort. Dividing by the size of the age cohort in year *t*, we calculate the relative size of the increase in migrant share for each age cohort. See Appendix Figure A.7 for the evolution of the share of each cohort working as migrants over time (panel A) and the age distribution of the village workforce (panel B).

Figure 7
What is the Direct Effect of Id Distribution on the Probability of Enrolling in High School?

Evidence from Reduced Form Estimates

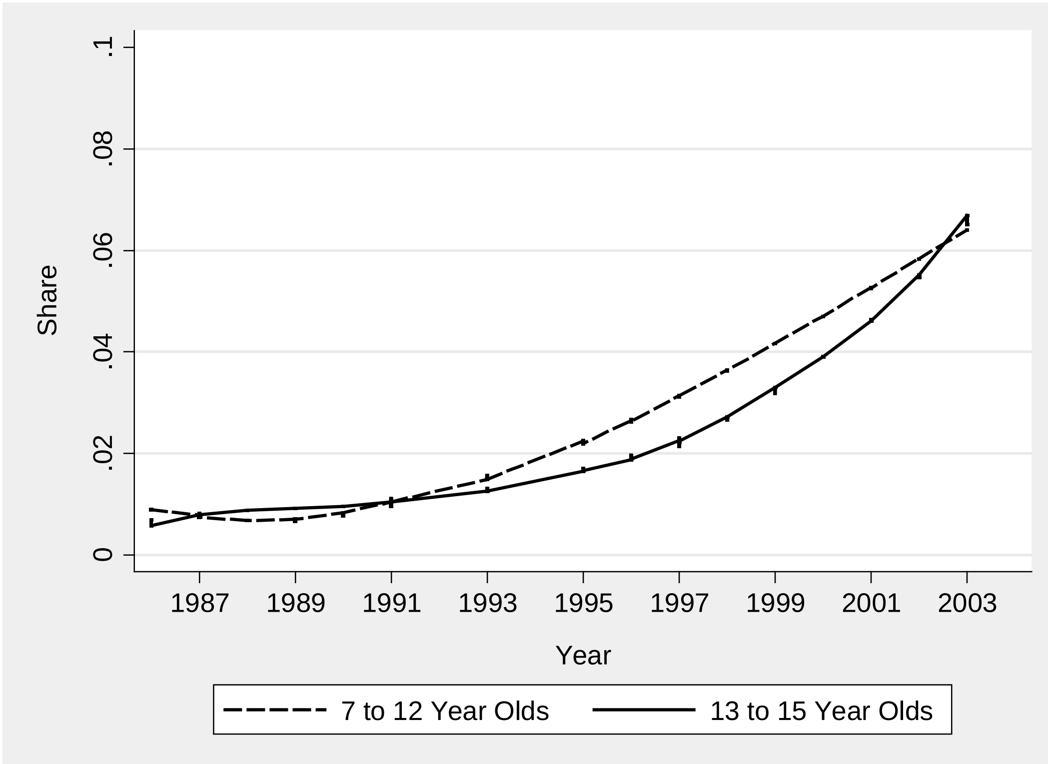
A. Reduced Form vs Years-Since IDs

B. Reduced-Form + Trend vs Years



Source: RCRE panel (1986-2003) and Supplemental Survey (2004).
Note: In panel A, the solid line shows the lowess plot of the predicted migrant share in the village workforce versus years since ID cards were issued, and the dotted line shows the predicted effect of years since ID cards were issued on the probability of enrolling in high school. This effect is predicted from the coefficients on the quartic in years-since IDs were distributed in equation (3) after village fixed effects, province-year fixed effects, household characteristics and time-varying village effects have been partialed out. The estimated coefficients are jointly significant at the 5 percent level, with a cluster corrected F-Statistic of 3.56. Panel B shows the observed rate of continuation onto high school by year (dotted line), the predicted rate of continuation based on village fixed effects and province-year fixed effects alone (dashed line), and the predicted rate of continuation when the quartic in years since IDs is added (solid line).

Figure 8
Share of Children who had a Migrant Parent During Their Childhood



Source: RCRE Supplemental Household Survey (2004)

Table 1
Educational Attainment of the Rural Registered Population
Evidence from the 2003 Rural Household Survey

Registered Population by Agricultural/Non-Agricultural/Migrant Distinction

	Activity and Location			
	Agricultural	Local, Non-Agricultural	Migrant	Total
Age	41.4	38.58	28.62	38.7
Male Share	0.46	0.63	0.63	0.52
Years of Education	7.35	8.98	8.87	7.87
Educational Attainment				
Illiterate	0.09	0.03	0.02	0.07
Primary	0.37	0.19	0.16	0.30
Middle School	0.45	0.55	0.67	0.51
High School	0.09	0.21	0.14	0.12
College	0.00	0.02	0.01	0.00
Observations	130533	30241	30936	191710

Source: National Bureau of Statistics (NBS) 2003 Rural Household Survey (Full National Sample), from Park et al (2006).

Table 2

Non-Parametric First-Stage (Migrant Share) and Reduced-Form (In School or Not?) Estimates

Dependent Variable	Migrant Share in Village		In school or not	
	(1)	(2)	(3)	(4)
4 or more years before cards issued	-0.022		0.026	
	(0.046)		(0.212)	
3 years before cards issued	-0.017		0.117	
	(0.025)		(0.137)	
2 years before cards issued	-0.023		0.099	
	(0.019)		(0.101)	
1 year before cards issued	-0.011		0.126*	
	(0.016)		(0.069)	
1 year after cards issued	0.008	-0.006	0.090	0.034
	(0.015)	(0.014)	(0.064)	(0.056)
2 years since cards issued	0.009	-0.013	0.013	-0.068
	(0.019)	(0.016)	(0.090)	(0.066)
3 years since cards issued	0.011	-0.014	-0.018	-0.058
	(0.024)	(0.016)	(0.114)	(0.063)
4 years since cards issued	0.042	0.014	-0.080	-0.133*
	(0.029)	(0.018)	(0.150)	(0.078)
5 years since cards issued	0.065**	0.032*	-0.077	-0.134
	(0.034)	(0.020)	(0.180)	(0.083)
6 years since cards issued	0.067*	0.028	-0.171	-0.224**
	(0.040)	(0.023)	(0.217)	(0.095)
7 years since cards issued	0.086*	0.043*	-0.075	-0.129
	(0.046)	(0.024)	(0.247)	(0.097)
8 years since cards issued	0.091*	0.042	-0.203	-0.255**
	(0.051)	(0.026)	(0.280)	(0.102)
9 years since cards issued	0.147**	0.093**	-0.217	-0.267**
	(0.057)	(0.028)	(0.316)	(0.114)
10 years since cards issued	0.123*	0.063**	-0.208	-0.258**
	(0.063)	(0.029)	(0.352)	(0.125)
11 years since cards issued	0.133*	0.068**	-0.155	-0.203
	(0.069)	(0.032)	(0.386)	(0.133)
12 years since cards issued	0.129*	0.058*	-0.259	-0.306**
	(0.076)	(0.034)	(0.422)	(0.144)
13 years since cards issued	0.140*	0.063*	-0.263	-0.308**
	(0.083)	(0.038)	(0.457)	(0.153)
14 years since cards issued	0.146*	0.063	-0.283	-0.326**
	(0.089)	(0.041)	(0.492)	(0.163)
15 years since cards issued	0.152	0.064	-0.213	-0.253
	(0.096)	(0.046)	(0.529)	(0.179)
16 years since cards issued	0.141	0.047	-0.174	-0.209
	(0.104)	(0.051)	(0.566)	(0.193)
17 years since cards issued	0.207*	0.108*	-0.011	-0.046
	(0.116)	(0.064)	(0.605)	(0.213)
18 years since cards issued	0.089	-0.015	-0.322	-0.357
	(0.117)	(0.057)	(0.653)	(0.258)
19 years since cards issued	0.262	0.152	-0.647	-0.673**
	(0.164)	(0.124)	(0.673)	(0.228)
Number of observations	3,160	3,160	3,160	3,160
F statistic, Years Prior to Card Issuance	0.48		1.25	
p-value, F statistic, Years Prior to Card Issuance	0.75		0.29	
F statistic, all variables	2.168	2.121	2.710	2.973
p-value, F statistic, all variables	0.002	0.003	<0.0001	<0.0001

Note: Standard errors in parentheses, clustered at the village-year level. *-indicates significance at the 10 percent level; **- indicates significance at the 5 percent level. The first F statistic tests the hypothesis that coefficients on all variables measuring years prior to card issuance are zero. The latter F statistic tests the hypothesis that coefficients on all variables are zero.

Table 3
What Factors Determine the Size of the Village Migrant Network?
Regressions Explaining the Share of Migrants in the Village Labor Force, Using the Sample of Individuals Completing Middle School, 1986-2003

	(1)	(2)	(3)	(4)
Dummy, Cards Issued > 3 Years Ago	0.033**			
	(0.012)			
1-2 Years Since Cards Issued		-0.007		
		(0.013)		
3-4 Years Since Cards Issued		0.0004		
		(0.015)		
5-6 Years Since Cards Issued		0.032		
		(0.018)		
7-8 Years Since Cards Issued		0.045*		
		(0.024)		
9-10 Years Since Cards Issued		0.082**		
		(0.027)		
11+ Years Since Cards Issued		0.071**		
		(0.030)		
Years Since Cards Issued			0.012**	-0.017**
			(0.004)	(0.009)
Years Since Cards Issued, Squared			-0.0003	0.007**
			(0.0002)	(0.003)
Years Since Cards Issued, Cubed				-0.001
				(0.0002)
(Years Since Cards Issued)^4/10				0.0002**
				(0.0001)
Number of observations	3,160	3,160	3,160	3,160
Cluster Corrected F Statistic	7.596	3.437	4.132	3.663

Notes: The F-statistic, corrected for clustering at the village-year level, tests the hypothesis that the estimated coefficients on the instruments are zero. *All F statistics are significant at the one percent level.* In parentheses, we show robust standard errors that allow for arbitrary correlation within village-years. *- indicates significance at the 10 percent level; **- indicates significance at the 5 percent level. All regressions control for factors related to village location with village fixed effects, and macroeconomic shocks using province-year fixed effects.

Table 4
Hazard Model for Distribution of ID cards

	Dependent Variable: 1 when card is issued; 0 otherwise					
	(1)		(2)		(3)	
	coefficient	marginal	coefficient	marginal	coefficient	marginal
Squared Rainfall Shock, lagged once	-2.433	-0.030			-2.439	-0.030
	(2.892)	(0.038)			(2.907)	(0.039)
Squared rainfall shock, lagged twice			0.356	0.004	0.385	0.005
			(3.126)	(0.039)	(3.108)	(0.038)
Number of Obs.	314		304		304	
Log Likelihood	-86.8		-89.5		-89.3	
Chi-Square Statistic					0.72	
p-value, est. coeffs. are jointly zero					0.698	

Notes: We alternatively use the square of the rainfall shock for the July to November period in year t-1 and year t-2, and combine them. Giles and Yoo (2007) analyze the crop calendar for these four provinces and demonstrate that large shocks during the July-November period (represented by the square of the deviation from average rainfall) are a strong predictor of a poor winter wheat harvest in the following year (driven by the effects of water-logging with heavy rain, or draught with little rain), and has a negative impact on the value of agricultural production, returns to local employment, and returns to agricultural production. Giles and Yoo (2007) show that one- and two-year lags of the squared rainfall shock are associated with increased out-migration, more days of work as migrants, and increases in migrant remittances. Provincial dummies and year dummies included in all equations. Hypothesis tests are chi-squared tests for the null hypothesis that all coefficients are jointly zero. Marginal effects are estimated at the mean values of the square of rainfall shocks. The shock is defined as the difference of current year rainfall over the July to November period from thirty year average of total rainfall during these months.

Table 5
Are the "Years-Since IDs" Instruments Correlated with Time-Varying Village Policies?
F-Statistics on Instruments (p-values)

	Explanatory Variables Included	
Policy Variable	Instruments (Quartic in Years Since ID Cards Issued)	Instruments + Household and Village Controls
Share of Grain Sold at Quota Price (Calculated by Value)	0.67	0.39
	(0.618)	(0.817)
Share of Grain Sold at Quota Price (Calculated by Weight)	0.42	0.50
	(0.796)	(0.739)
Share of Village Land Planted in Orchard Crops	2.05	0.63
	(0.102)	(0.640)
Share of Households Renting In Land	0.85	1.01
	(0.501)	(0.413)
Share of Households Renting Out Land	1.20	1.08
	(0.323)	(0.378)
Average Village Per Capita Local Tax Rates Paid by Households	0.73	0.67
	(0.574)	(0.619)

Notes: Each policy variable listed is the dependent variable in regression models and we report the F-Statistic for the hypothesis that the coefficients on the quartic in years since IDs were issued are jointly zero. The number in parentheses is the p-value for the F-statistic. All models include village and province-year dummy variables and standard errors are robust to within village correlation of residuals.

Table 6
Regression of Dummy Variable for High School Enrollment on Migrant Share in Village

Specification of Instrument	Non-Parametric	Dummy Variable, >3 years	Groups of Dummy Variables	Quadratic, Years Since IDs Issued	Quartic, Years Since IDs Issued
	(1)	(2)	(3)	(4)	(5)
IV Estimator, Correcting for Clustering					
Share of village employed in migrant activities	-0.521	-3.192*	-1.524*	-3.989**	-2.503**
	(0.576)	(1.780)	(0.793)	(1.872)	(1.061)
IV GMM Estimator (Includes Weighting Matrix for Efficiency)					
Share of village employed in migrant activities	-0.775	-3.192*	-1.423**	-3.813**	-2.434**
	(0.532)	(1.780)	(0.789)	(1.838)	(1.040)
Test Statistics					
Angrist-Pischke F Statistic, instruments	2.168	7.596	3.437	4.132	3.663
Hansen J Statistic (overidentification)	29.689		1.723	0.247	1.912
p-value, J statistic	0.041		0.886	0.619	0.591
Anderson-Rubin F statistic, weak instruments		6.61	6.24	10.59	12.31
p-value Anderson-Rubin F statistic		0.010	0.397	0.005	0.015
Conditional Likelihood Ratio (CLR) Test			4.68	10.55	10.98
p-value, CLR test			0.052	0.002	0.002
95% Confidence Interval, based on CLR Test			[-4.52, -0.04]	[-11.39, -1.38]	[-6.67, -0.95]
LIML Estimator					
Share of village employed in migrant activities	-0.648	-3.192*	-1.552*	-4.093**	-2.612**
	(0.746)	(1.780)	(0.811)	(1.947)	(1.126)
Test Statistics					
Cluster Corrected F Statistic, instruments	2.168	7.596	3.437	4.132	3.663
Hansen J Statistic (overidentification)	29.689		1.723	0.247	1.912
p-value, J statistic	0.041		0.886	0.619	0.591
Anderson-Rubin F statistic, weak instruments		6.61	6.24	10.59	12.31
p-value Anderson-Rubin F statistic		0.010	0.397	0.005	0.015
Conditional Likelihood Ratio (CLR) Test			4.68	10.55	10.98
p-value, CLR test			0.052	0.002	0.002
95% Confidence Interval, based on CLR Test			[-4.55, -0.03]	[-11.77, -1.38]	[-7.03, -0.97]
Number of Observations	3160	3160	3160	3160	3160

Notes: Column 1 corresponds to the instrument as specified in column 2 of Table 2; columns 2-5 correspond to the instrument as specified in columns 1-4 of Table 3. Standard errors clustered at the village-year level, except in the Moreira conditional IV estimator, where they are robust. All regressions control for factors related to village location with village fixed effects, and macroeconomic shocks using province-year fixed effects. To test for weak identification, we use the cluster corrected F statistic. We use the Hansen J statistic to test for overidentification. For weak instrument robust inference, we use two test statistics. The Anderson-Rubin F statistic tests the hypothesis that the instruments and the coefficient on the endogenous variable are jointly zero. Under the assumption of homoscedasticity, the conditional likelihood ratio (CLR) test developed by Moreira (2003) is more powerful, so we also show it and the 95% confidence interval suggested by the CLR test. The Anderson-Rubin and CLR tests are generalized for clustered dependence in error terms using the minimum distance approach by Finlay and Magnusson (2009). The test statistics do not differ whether or not we use the weighting matrix for efficiency with instrumental variables, so we present them once.

Table 7
Determinants of High School Enrollment
Conditional on Completing Middle School, 1986-2003

	Dependent Variable: Enroll in High School Next Year = 1						
Model	(0)	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV-GMM	IV-GMM	IV-GMM	IV-GMM	IV-GMM	IV-GMM
Share of Migrants in Village	-0.097	-2.434**	-2.217**	-1.981**	-1.868**	-1.801**	-2.237**
Labor Force	(0.132)	(1.040)	(0.914)	(0.877)	(0.843)	(0.836)	(1.060)
Gender (1=male)				0.045**	0.051**	0.050**	0.050**
				(0.019)	(0.018)	(0.018)	(0.019)
First Born (1=yes)				0.043**	0.023	-0.030	-0.029
				(0.019)	(0.019)	(0.027)	(0.027)
First Born in Household				-0.072**	-0.082**	-0.069**	-0.072**
was Male (1=yes)				(0.019)	(0.019)	(0.020)	(0.021)
Father's Years of Schooling					0.022**	0.022**	0.022**
					(0.004)	(0.004)	(0.004)
Mother's Years of Schooling					0.026**	0.026**	0.026**
					(0.004)	(0.004)	(0.004)
Number of Potential Migrants,						-0.053**	-0.053**
Household, Male						(0.020)	(0.020)
Number of Potential Migrants,						-0.028	-0.030
Household, Female						(0.018)	(0.018)
ln(Village Mean Income Per Capita)			0.045	0.039	0.040	0.042	<0.001
			(0.066)	(0.064)	(0.062)	(0.062)	(0.071)
Village Total Land			0.001	0.001	0.001	0.001	<0.001
			(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Village Cropped Land Per Capita Gini			-0.056	-0.068	-0.111	-0.132	-0.135
			(0.314)	(0.299)	(0.285)	(0.282)	(0.308)
(Village Labor Force)/10			-0.001	-0.001	-0.001	-0.001	-0.001
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Cultivable Share of Village Land			0.012	0.018	-0.039	-0.033	-0.042
			(0.139)	(0.133)	(0.127)	(0.126)	(0.137)
Additional Time-Varying Village Variables?	no	no	no	no	no	no	yes
Chi-Square Test, Time-Varying Village Variables							4.57
p-value, Chi-Square Test							0.600
Over-ID Test: Hansen J-Statistic		1.912	2.584	2.880	2.468	2.484	3.567
P-value, J-statistic		0.591	0.460	0.410	0.481	0.478	0.312
Cluster corrected F Statistic, instruments		3.663	4.400	4.462	4.462	4.461	3.141
CLR Test Statistic		10.98	11.02	9.47	8.82	8.35	10.71
CLR Test, p-value		0.002	0.002	0.004	0.006	0.007	0.003
Number of Obs.	3160	3160	3157	3157	3157	3157	3157

Notes: In parentheses, we show standard errors clustered at the village-year level. All regressions control for factors related to village location with village fixed effects, and macroeconomic shocks using province*year fixed effects. Additional time-varying village level variables in model 6 include the average proportion of households with non-agricultural self-employment, the share of grain sold at quota, the logarithm of average household wealth, the share of land allocated to aquaculture, the share of land allocated to forestry, and , the share of land allocated to orchards. Models 1-6 are estimated using an instrumental variables-generalized method of moments estimator that is efficient in the presence of presence of heteroskedasticity and arbitrary within village-year cluster correlation (see Wooldridge 2002, page 193).

Table 8
Descriptive Statistics, Main Estimation Sample

Variable	Mean (Standard Deviation)
Enrolled in High School? (1=yes)	0.429
	(0.495)
Share of Migrants in Village Workforce	0.109
	(0.115)
Years Since IDs Issued	7.25
	(5.10)
Logarithm, Mean Village Income	6.42
	(0.39)
Gini Ratio, Cultivated Land, Village	0.207
	(0.080)
Total Land, Village (mu)	50.9
	(57.1)
Share of Land, Village, Arable	0.580
	(0.282)
Gender (1=male)	0.567
	(0.495)
First Born Child? (1=yes)	0.454
	(0.498)
First Born Child in Household was Male (1=yes)	0.399
	(0.490)
Father Years of Schooling	6.39
	(3.21)
Mother Years of Schooling	4.22
	(3.30)
Potential Migrants, Male	0.460
	(0.625)
Potential Migrants, Female	0.488
	(0.711)
Share of Land, Village, in Forests	0.155
	(0.269)
Share of Land, Village, in Orchards	0.047
	(0.079)
Share of Land, Village, in Fish Ponds	0.043
	(0.058)
Percent of Households with Non-Agricultural Labor, Village	0.555
	(0.278)
Logarithm, Wealth per Capita, Village	8.81
	(0.55)
Share of Village Grain sold at Quota Price	0.086
	(0.083)
Father has professional employment (1=yes)	0.051
	(0.221)
Father has off-farm employment (1=yes)	0.171
	(0.377)
Father enrolled in high school (1=yes)	0.117
	(0.322)
Father completed high school (1=yes)	0.087
	(0.281)

Notes: Standard deviations in parentheses.

Table 9
Does the Effect of Migrant Opportunity Differ with Family Characteristics?

	Dependent Variable: Enroll in High School=1				
Model	(1)	(2)	(3)	(4)	(5)
Migrant Share of Village Labor Force	-2.148**	-2.059**	-1.522*	-1.980**	-2.212**
	(0.907)	(0.869)	(0.764)	(0.912)	(0.874)
Father is Professional (1=yes)	0.137**	0.318**	0.094*	0.142**	0.141**
	(0.049)	(0.082)	(0.052)	(0.050)	(0.050)
(Migrant Share)*(Father is Professional)		-2.006**			
		(0.826)			
Father had Off-Farm Work (1=yes)	-0.035	-0.043	0.099	-0.037	-0.037
	(0.032)	(0.032)	(0.066)	(0.032)	(0.032)
(Migrant Share)*(Father had Off-Farm Work)			-1.008**		
			(0.479)		
Father Enrolled in High School (1=yes)	0.169**	0.175**	0.174**	0.272**	0.174**
	(0.031)	(0.031)	(0.031)	(0.052)	(0.031)
(Migrant Share)*(Father Enrolled in High School)				-0.922**	
				(0.379)	
Father completed High School (1=yes)	0.033	0.040	0.034	0.037	0.091
	(0.040)	(0.041)	(0.040)	(0.041)	(0.071)
(Migrant Share)*(Father Completed High School)					-0.461
					(0.509)
Hansen J statistic	1.12	3.01	3.95	5.18	2.37
p-value, J statistic	0.773	0.808	0.684	0.521	0.883
F statistic, Number of Migrants	4.46	3.28	3.95	5.18	3.23
F statistic, interaction term instruments=0		12.74	19.33	28.67	14.19
Number of Observations	3006	3006	3006	3006	3006

Notes: In parentheses, we show robust standard errors clustered at the village-year level. All models control for factors related to village location with village fixed effects, and macroeconomic shocks using province-year fixed effects, as well as village and individual level controls listed in column 5 of Table 7. All models are estimated using an IV-GMM estimator that is efficient in the presence of heteroskedasticity and arbitrary within village-year cluster correlation (see Wooldridge 2002, page 193).

Table 10
Estimated Effect of Village Migration on Youth Activity Choice

	Activity Choice Next Year:			
	In School?	Participate in Agricultural or Home Labor	Participate in Local Wage Labor	Participate in Migrant Labor Market
Age to Complete Middle School this Year	-2.028*	0.302	0.106	0.116
	(1.067)	(0.986)	(0.150)	(0.290)
Age to Complete Middle School last Year	-0.970	-1.229	0.692**	0.739**
	(1.065)	(1.339)	(0.301)	(0.286)
Age to Complete Middle School Two Years Ago	-0.972	-3.066**	0.845**	0.906**
	(0.996)	(1.398)	(0.430)	(0.323)

Notes: In parentheses, we show robust standard errors clustered at the village-year level. All regressions control for factors related to village location with village fixed effects, and macroeconomic shocks using province-year fixed effects, as well as village and individual level controls listed in column 5 of Table 7. All models are estimated using an IV-GMM estimator that is efficient in the presence of heteroskedasticity and arbitrary within cluster correlation (see Wooldridge 2002, page 193).

Appendix Discussion and Additional Tables and Figures

for

Migrant Opportunity and the Educational Attainment of Youth in Rural China

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The following pages include additional documentation on enrollment patterns and duration of schooling, a discussion of returns to education among migrants and results from estimating the impact of migration on the probability of high school enrollment using an alternative identification strategy. Also included is supplementary evidence in additional appendix figures and tables referenced in the body of the main paper.

A.1. Evidence on Age of School Enrollment, Duration in School and Incidence of Repeated Grades

China's Compulsory Education Law, passed in 1986, mandated that all children enroll in school at age six regardless of gender, but allows enrollment to be postponed to age seven in those localities (urban and rural) where enrollment at age six is not feasible.[1] In its aspiration for early start dates, the implementation of the Compulsory Education Law bears striking similarity to other legal and institutional reforms in China (ranging from governance over land, to employment protections and protections over intellectual property, and as exploited in this paper, the distribution of national identity cards). In education, as in all of these other areas, legislation is used to set out a future national goal, but provinces, counties and sometimes even local communities (villages and urban neighborhoods) are granted flexibility to implement these changes as local conditions evolve. The high incidence of an age seven start date for school enrollment is evident from both census data and other surveys conducted in the early 2000s. China's 1990 national census shows that only 17 and 16 percent of male and female rural children who turned 6 by August 1990, respectively, were enrolled in school (Appendix Figure A4). By contrast, the enrollment rates of seven year olds were 60 and 55 percent for boys and girls, respectively.

We also note that data from a six-province survey collected by the Chinese Center for Agricultural Policy (CCAP) in 2000 lends further support to our assumption that children start school at age seven. Geographically, the CCAP sample overlaps with our sample as it is drawn from the same region of the country.[2] The CCAP survey form asked respondent household members to report the age each child began primary school, and the modal year of respondents answering in 2000 was seven (Appendix Table A.3). CCAP repeated their survey (in the same villages, with the same form) in 2009, and the modal answer was again seven. Another well-known survey (The Gansu Study of Families and Children) focusing on education in a poorer province, Gansu, enumerated the first grade start date in rural households as well, and the overwhelming majority of students surveyed in 2000 and 2004 also reported starting elementary school at age seven.

[1] 《中华人民共和国义务教育法》(1986年)，第五条，凡年满六周岁的儿童，不分性别、民族、种族，应当入学接受规定年限的义务教育。条件不具备的地区，可以推迟到七周岁入学。(Compulsory Education Law of the People's Republic of China, 1986, Article 5. All children who have reached the age of six shall enroll in school and receive compulsory education for the prescribed number of years, regardless of sex, nationality or race. In localities where this is not possible, enrollment may be postponed to the age of seven.) The Articles of Compulsory Education Law could be found here (both the English version and the Chinese version): http://www.lawinfochina.com/display.aspx?lib=law&id=1166&CGid.

[2] See de Brauw et al (2002) for a description of the CCAP survey.

By the time of the 2000 population census, progress on enrolling children in elementary school was significant, with 72 and 71 percent of boys and girls who were then age six, respectively, enrolled in school. Thus, between 1990 and 2000 there was considerable expansion of rural schools and success in reducing the age at which children started school to six, but most of the children in the RCRE sample used in the analysis of this paper were born earlier. The youngest students in the RCRE sample were of age to finish middle school in 2002 and decide about whether to enter high school or not in 2003, and these students would have entered first grade by 1994. Only 18 percent of our sample entered elementary school between 1991 and 1994, while the other 82 percent started primary school in 1990 or before.

Evidence on the timing of efforts to meet goals for lower enrollment ages suggests that the change was not smooth between 1990 and 2000. Efforts to lower enrollment rates were pursued in urban areas first, and then much of the focus on rural school enrollment ages occurred after the mid-1990s with other efforts to expand services in rural and more remote areas. Given evidence from the CCAP surveys, the Gansu Study of Families and Children, and enrollment rates at age six and seven from the 1990 census, there appears to be ample evidence to support our assumption that for most children in our sample, the first year of primary school enrollment was at age seven.

There will be some variation around this starting age, and as suggested by one of our critical readers, it is important to think about both how it will affect our estimates and the robustness checks that are worth performing. For identification, it is most important that our instrument for migrant employment is uncorrelated with unobserved differences in ability. First, phase in of the compulsory education law suggests that much variation in starting age is at the community level, and this is evident in the data. This dimension of variation is unrelated to student ability. Second, students starting school at age six or age eight likely make decisions about entering high school one year before or one year after the date we assume in this paper. We know whether the decision to enroll in another year of schooling was made, and being off by a year will mean that the predicted increase in migrant share from the village will not be perfectly mapped into the year of the decision. The quartic in years-since-IDs is a smooth function and we do not see extremely sharp jumps in the predicted migrant share variable from one year to the next, so it is unlikely that the instrument is systematically related to within village variation in unobserved ability, or that it will introduce bias to our analyses.

A second assumption on the duration of primary school is important for assigning the year of potential high school enrollment. In some parts of rural China, primary school lasted for five years for much of the period, whereas other regions achieved the six-year mandate much earlier. The RCRE supplemental survey did not directly ask whether villages have five or six year primary schools, but when we examine completed years of schooling at the village level, it is fairly straightforward to discern whether completed schooling patterns are consistent with five or six year primary schools. We found that in some villages most children completed either 6, 9 or 12 years of school; as middle and high school each last three years, and these patterns were consistent with six year primary schools. In other villages, most children completed 5, 8, or 11 years of schooling, consistent with five year primary schools.[3] Using this information, we coded all of the villages as five or six year primary school villages. To illustrate our assumption, we show average enrollment rates for each grade level in five and six year primary school villages conditional on completing the previous grade (Table A.4). Until the decision to enter grade 9 or grade 10, transition probabilities from one grade to the next are well over 90 percent. The first significant decision node is clearly either the decision to move from grade 8 to grade 9 (in five year villages) or from grade 9 to grade 10 (in six year villages). We measure the decision to enroll in high school with a variable that includes the decision to enter grade 10 conditional on completing grade 9 for six year primary school villages and the decision to enter grade 9 conditional on completing grade 8 for five year primary school villages.[4]

A final concern involves handling repeated years of schooling or skipped grades. Although the supplemental survey did not ask explicitly about repeats or skips, the protocol for the supplemental survey required respondents to report years of schooling completed and the common interpretation is to answer in terms of the level of schooling completed. Examination of the CCAP data, which asked explicitly about repeats, suggests their inclusion does not affect the general distribution of educational attainment. Therefore our findings should be robust to any errors in the measurement of years of schooling.

A.2. An Alternative Instrument for Migration: GDP Growth in Potential Migrant Destinations as a Proxy for Labor Demand

An alternative approach to identifying migration involves using labor demand, or shocks at migrant destinations.[5] Measures of labor demand may identify effects of migration in sending

[3]In the one village in which our method was indeterminate, we assumed that the village had a five year primary school. Our results are robust to recoding the village as one with a six year primary school.

[4]All of our estimation results are robust to analyzing the grade 10 enrollment decision conditional on grade 8 completion.

[5]Several researchers have recently used variants of this approach. In looking at the effects of parent migration on child time allocation in Mexico, Antman (2011) uses shocks to weighted city-level employment in sectors thought to employ migrants in destination cities. Yang and Martinez (2006) and Yang (2008) use exchange rate shocks to identify the effects of remittances to the Philippines on household investment, poverty headcounts, and agricultural production, respectively. In all three cases, the authors weight the destination unemployment rate or exchange rate by the fraction of migrants from the source going to specific destinations.

communities if they only affect other household level outcomes through their effect on migrant labor supply decisions. While the prospective migrant observes expected wages at a set of potential destinations, and then uses that information to choose where to migrate, the full set of wage offers and potential destinations faced by any individual migrant are unobserved by the analyst. Therefore, implementing this approach involves assumptions regarding potential destinations and use of a proxy for labor demand.

First, both the RCRE surveys and the 1995 and 2005 agricultural censuses suggest that nearly 70 percent of rural migrants find work within their home provinces, we matched villages to the nearest city outside their home counties. In using the nearest city, we pick up the nearest urban labor market signal for potential migrants, and implicitly assume that it would be the best potential proxy for news about labor demand for young, first-time migrants. We use measures of city level GDP from the National Bureau of Statistics (CNBS), and construct year-to-year city-level growth rates.[6]

We test several potential specifications of instruments based on city GDP growth rates. Signals of labor market conditions may be picked up over the year and could show up in either lagged or current growth in city GDP. Thus, it is unclear whether year on year growth measured in year t, or $t-1$, is exerts stronger influence on the migration decision, and so we tried both. The strength of the growth signal might also depend upon the distance from the village to the nearest city, but as it is not clear that the relationship between distance and signal strength would be linear, we tested four different relationships between GDP growth and distance. First, we used the GDP growth rate in the nearest city itself, without controlling for the distance to the nearest city. Second, we divide the GDP growth rate by the distance, and in the third specification, we divide by the distance squared.

None of the above distance weightings above account for the fact that the effects of signal may vary non-linearly with distance. Individuals who live in relatively close proximity to a growing urban area may be able to commute for work, as opposed to migrating, even if the city is located in another county. Alternatively, after some distance X from the village, the effect of signals related to economic growth may decline more rapidly with distance. We thus posit that city GDP growth might have its strongest pull on village residents at that distance X, and that

[6] While we would prefer to use information on employment by industry sector, these data series were incomplete for many cities and in many years, and further, only include formal sector employment, and migrants are frequently employed in the small scale informal sector. For these reasons, we make use of the more complete city-level GDP from the series, from which we construct year to year growth rates.

this effect will decline for values greater or less than X. We therefore also experiment with an instrument that is defined as the growth rate divided by the squared difference between the distance D from the village to the nearest city and X, or $\frac{g}{(D-X)^2}$.

To test the plausibility of the various instrument specifications, we regress the share of the migrants from the village on each instrument described above, along with a full set of village dummies, and a full set of province-year dummy variables (Appendix Table A.8, columns 1-3). When using the contemporaneous growth rate, we use data from 1991 to 2003, and when we use lagged growth, we use data from 1993 to 2003. In the table, each cell represents a separate regression, and we list the numerator in the row (either contemporaneous or lagged GDP growth) and the denominator in the column. We find a significant relationship between the migrant share and the lagged GDP measure, but only when we control for distance either linearly or quadratically in the denominator. However, the F-statistics suggest that these would be very weak instruments and unlikely to be appropriate for estimating the second stage.

We next explore whether there is an optimal distance X from the village in which the employment signal would be strongest, as discussed above. We re-specify the denominator as $(X-D)^2$, and we perform a grid search over possible values of D to find the strongest relationship between the share of migrants in the village workforce and the instrument. For both GDP growth and lagged GDP growth, we find a strongly significant relationship between the migrant share and the instrument for values of D centered around 100 km. As it is generally implausible that rural residents could commute this distance over the period under study, or even much more than 25 km, this finding makes intuitive sense. We thus use a value of D=100km in column 4 of Appendix Table A.8.

We find that the growth instrument has the strongest relationship with the share of migrants from the village when we use contemporaneous growth. The cluster robust F-statistic is 7.64, which is below 10, but stronger than in other specifications. While one might prefer this specification based on instrument strength, we had some expectation that migration should follow the previous year's growth.

Thus, we first report the second stage result with no additional covariates and using lagged city GDP growth (Appendix Table A.9). When no additional control variables are included, the estimated coefficient is negative and significant at the 1 percent level (column 1). Perhaps most importantly, the point estimate (-2.12) is quite similar to the point estimate reported in Table 10 with no covariates (-2.50), and is within the range found when also controlling for additional covariates. From the perspective of demonstrating the validity of the quartic in years since IDs were issued as an instrument, this result offers some confirmation as the result is similar using a different instrument.

When we add covariates to the regression (columns 2-5), the estimated coefficient on migrant share remains negative; however, it is no longer significantly different from zero. Point estimates are slightly smaller in magnitude than the point estimates found using the quartic in years since IDs were issued, but they are consistent with estimates in Tables 6 and 7. Apart from levels of significance, the Anderson-Rubin test statistics indicate that we cannot reject the joint hypothesis that the instrument does not affect the migrant share and that the migrant share does not affect school enrollment. When we use the quartic in years-since-IDs were issued, this test is consistently rejected.

We can be somewhat more sanguine about results using contemporaneous city GDP growth in potential destinations (Appendix Table A.10). Estimated coefficients on the share of the village population working as migrants range from -2.16 to -2.28, and the Anderson-Rubin test statistics suggest that while weak, the instruments are sufficiently strong and yield results consistent with the years-since-IDs instruments. As these results provide no evidence against the years-since-IDs instrument, we view them as offering confirmation. Yet, as they identify effects of migration over short distances, we view this instrument as identifying the effects of relatively local migration. We prefer the years-since-IDs instrument used in the main text as it identifies the network outside the village without strong assumptions about the work locations of current and past migrant residents from the village.

A.3. Evidence on Returns to Education among Migrants from RCRE Villages

To understand how returns in the urban labor market might in influence decisions about enrollment in high school, we use a module from the 2003 round of the RCRE survey that was designed to study the returns to migrant employment. For individuals who had out-migrated from RCRE households in 2003, the RCRE survey collected information on earnings, the cost of migration and the number of days individuals worked as migrants. Using a sample of all adult children of the household head and spouse who were between 15 and 50 years old, we estimate the net returns to education for migrants using a Heckman selection model. Our objective is to assess whether the pattern of returns to education are consistent with the observed decline in high school enrollment in those villages where it became easier to migrate. For the selection equation, we use household land per capita and demographic characteristics (e.g. household size, number of laborers, number of elderly in the household, the household dependency ratio, the male/female ratio and number of children under 5). On average, we find that an additional year of education has a return of 2.9 percent (Appendix Table A.11, model 1).

To separately estimate the returns to years of schooling for primary and middle school, high school, and post-high school education, we introduce a linear spline in model 2. We find a higher return to primary, middle school and post-secondary years of schooling than to high school. Specifically, we estimate an average return of 4.0 percent to primary and middle school, but only a statistically insignificant 0.3 percent return to a year of high school (Appendix Table A.11, model 2). The return to post-secondary education, consistent with findings Heckman and Li (2004), is higher at 4.7 percent, but also statistically insignificant.

These estimated returns to schooling are much below recent estimates of returns among urban workers, may be biased downward as family member estimates of daily wages of migrants are likely to be imprecisely estimated.[7] Moreover, the selection coefficient on years of high school education is negatively related to whether an individual is a migrant or not, implying that individuals who stay in the village are more likely to go to high school. Returns to high school for rural migrants in urban areas are low, while individuals with a high school education in rural areas are able to qualify for more lucrative positions in village or township government or as managers (or owners) of local enterprises.

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[7]Parents may not know how much of the time children living away from home were working, and may not have precise information on earnings.

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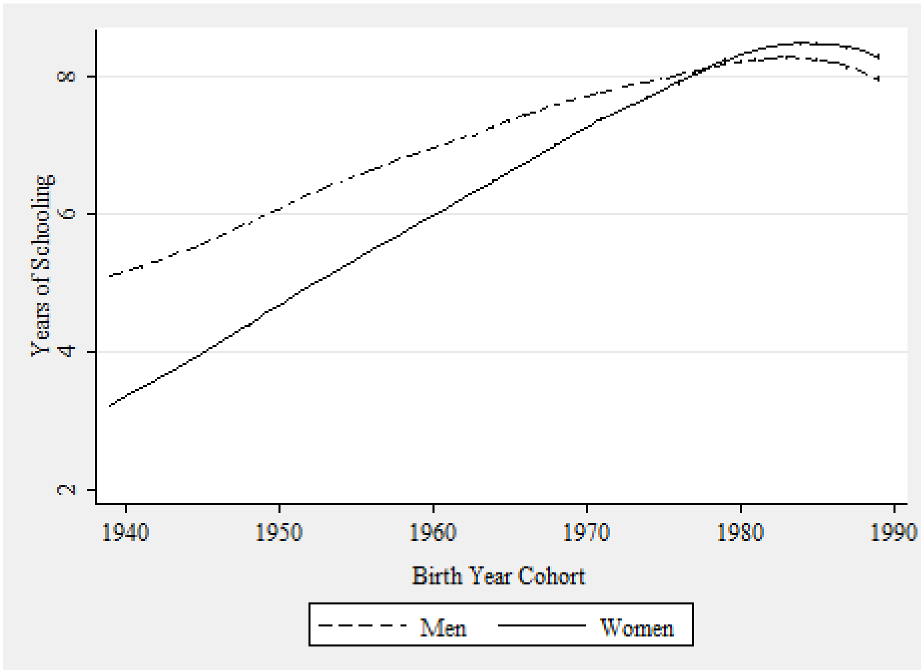
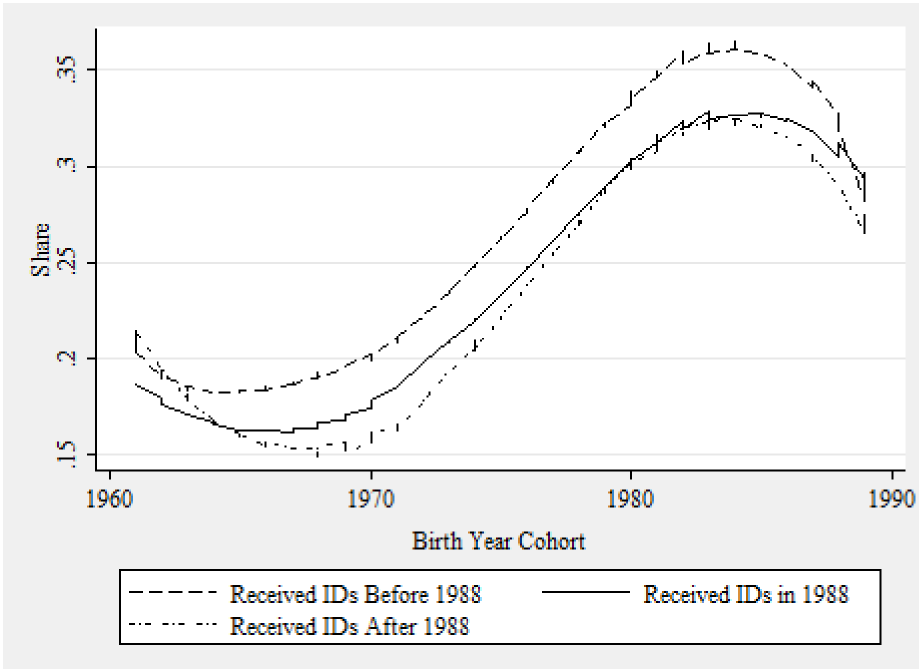
Figure A.1
Cohort Average Educational Attainment
Lowess Fit



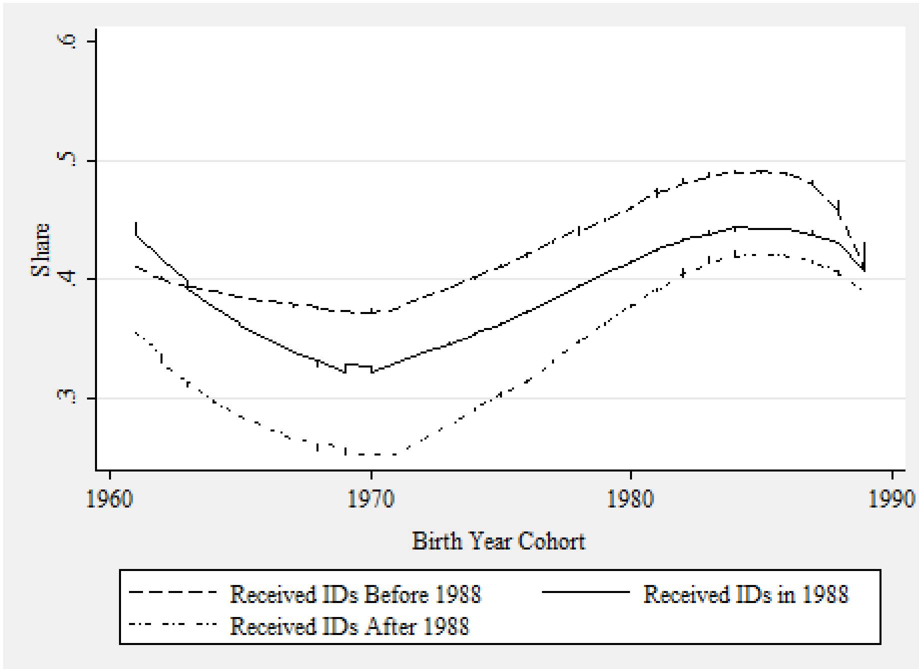
Source: RCRE Supplemental Survey (2004).

Figure A.2
Share of Age Cohort Entering High School
by Timing of ID Card Receipt
Lowess Fit



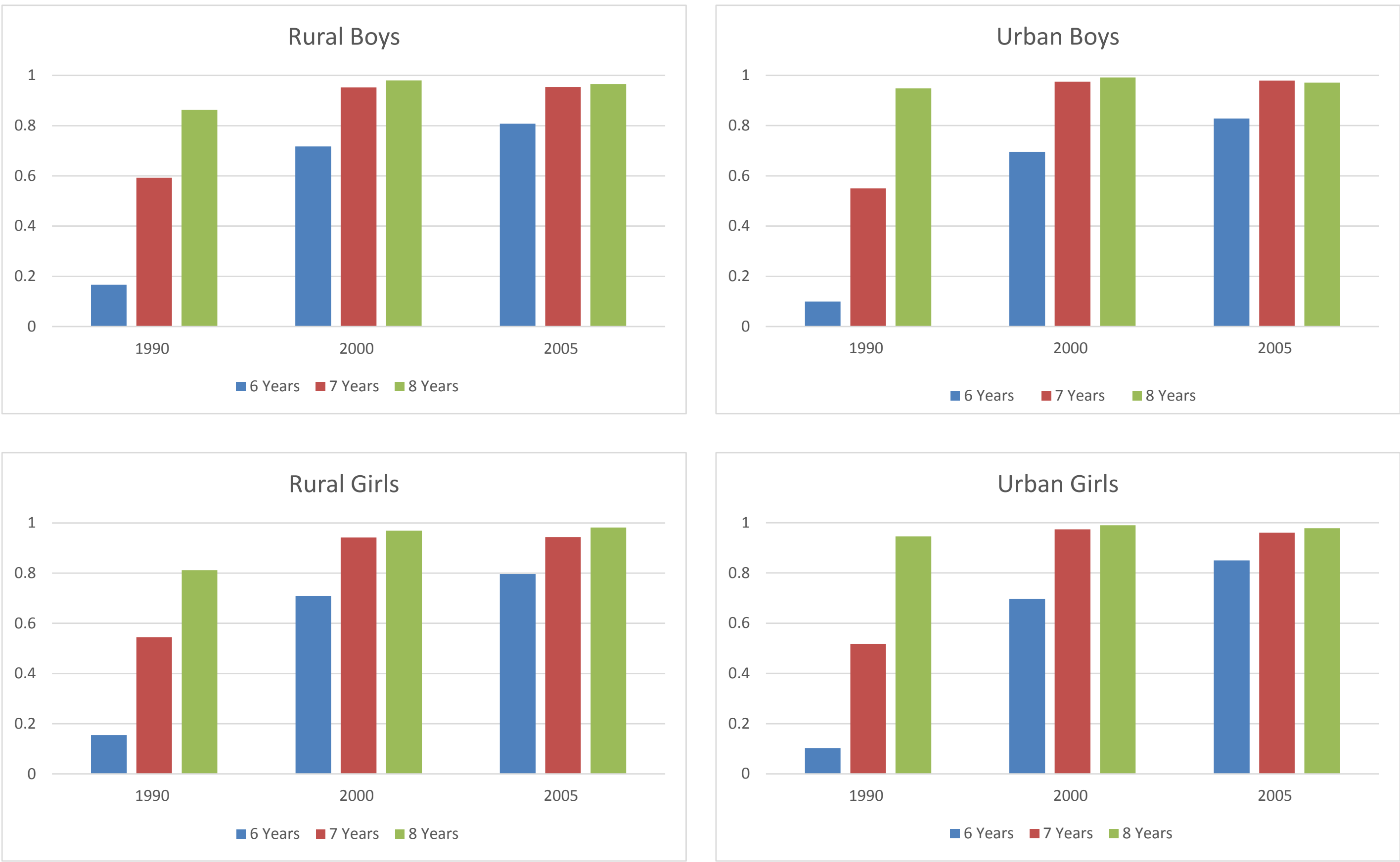
Source: RCRE Supplemental Survey (2004).

Figure A.3
Share of Middle School Graduates Entering High School
By Timing of ID Card Receipt
Lowess Fit



Source: RCRE Supplemental Survey (2004).

Figure A.4
Evidence from the Census on School Enrollment Rates of Young Children
Aged 6, 7 and 8

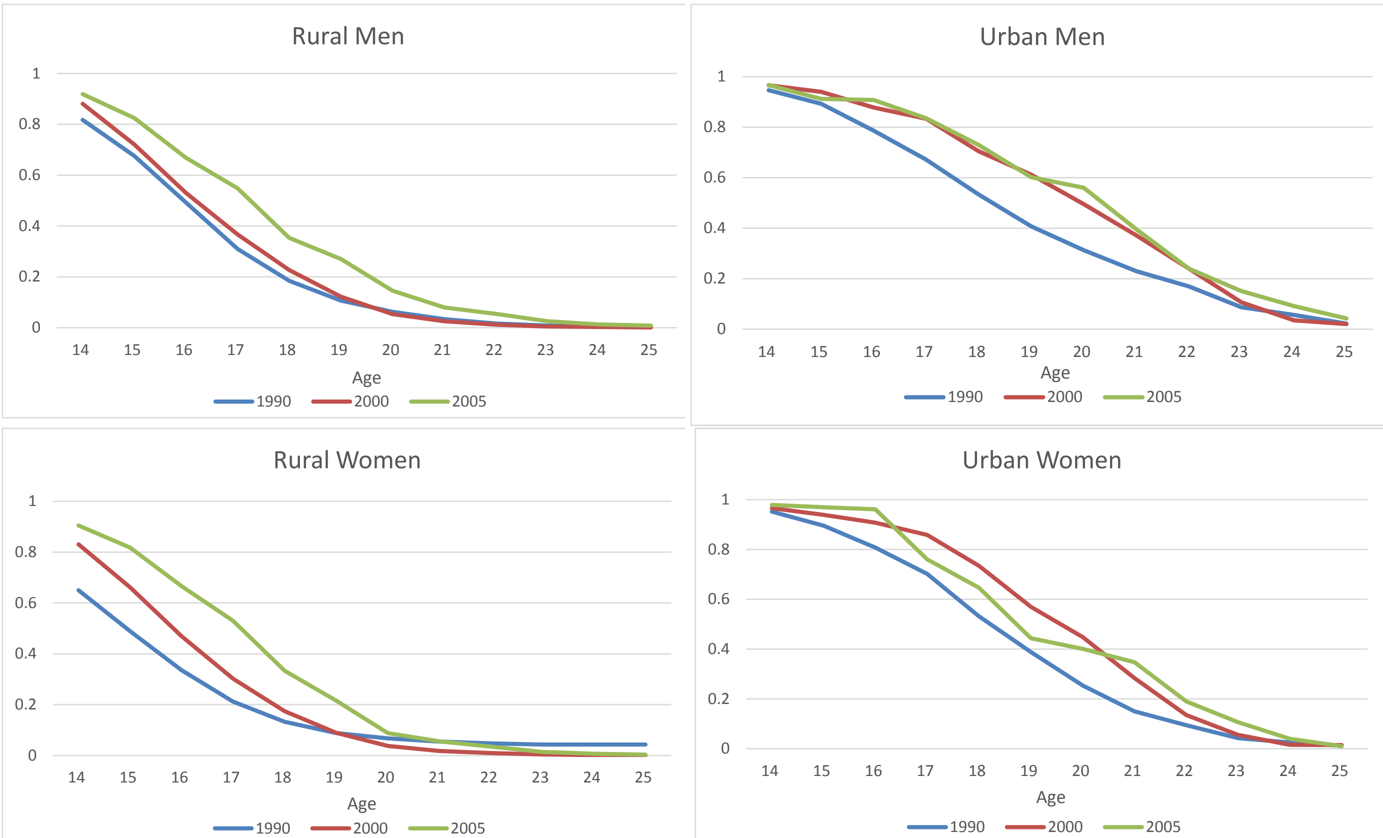


Sources: 1990 and 2000 Population Census and 2005 One Percent Population Sample. National Bureau of Statistics (Beijing).

Figure A.5
Educational Attainment Migrants and Local Urban Workers Aged 22-25
Evidence from Population Census Data on Educational Attainment

Source: Population Census (1990, 2000) and 2005 Population Sample (2005), National Bureau of Statistics (Beijing).

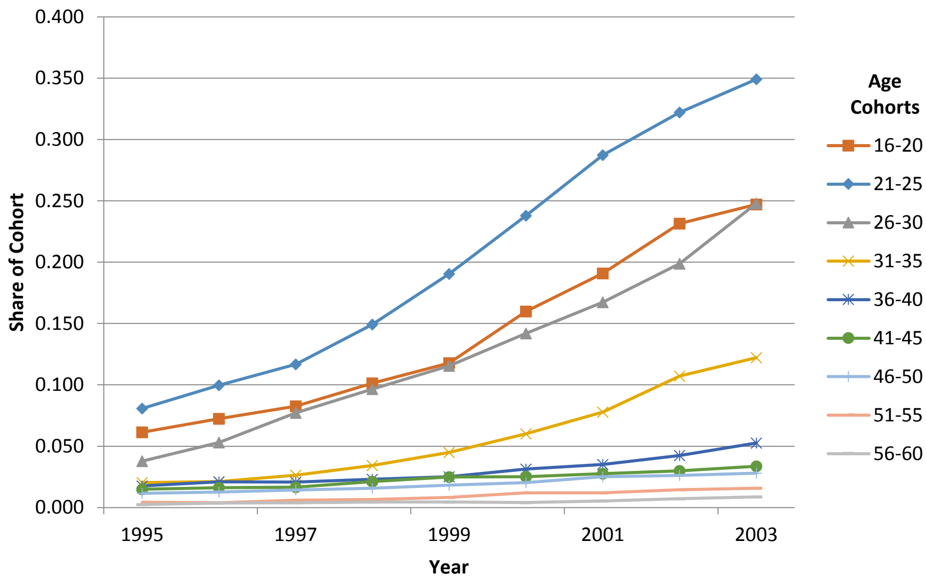
Figure A.6
Educational Enrollment Rates of Teens and Young Adults by Residential Registration (*Hukou*)
Evidence from the Population Census



Note: Enrollment may be in urban or rural areas, these rates are calculated as share of registered rural and urban population enrolled regardless of current residence location.
Source: 1990 and 2000 Population Census and 2005 Population Sample, National Bureau of Statistics (Beijing).

Figure A.7
Evidence on the Age Structure and Working-Age Adults in RCRE Villages

A. Migrant Share of Five-Year Age Cohorts



B. Five-Year Age Group as a Share of the Registered Village Population

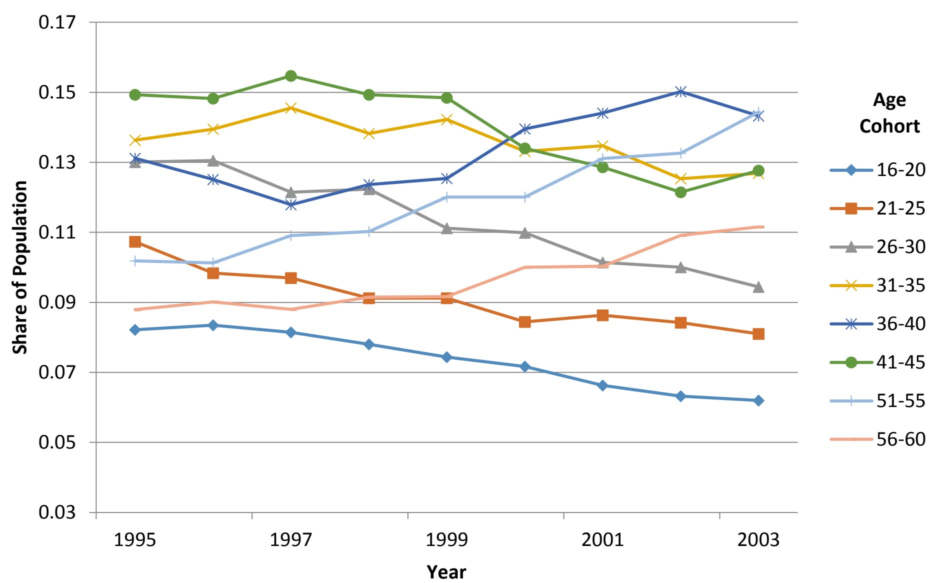


Table A.1
Local Networks of Rural-Urban Migrants at Time of Migration
Five-City China Urban Labor Survey (CULS) Migrant Survey*

	Source Community Location	
	All Provinces	4 RCRE Provinces
Share of Migrants with:		
Job Arranged Before *First* Migration Experience	0.52	0.57
Job Arranged Before *Current* Migration Experience	0.53	0.56
Some Acquaintance from Home Village in City Before Migrating	0.91	0.94
**Close Family Member in City Before Migration	0.35	0.35
**Extended Family Member in City Before Migration	0.52	0.58
**Hometown Acquiantances	0.65	0.67
Five or Fewer Hometown Acquaintances	0.39	0.44
More than Five Hometown Acquaintances	0.27	0.24
At Least One Local Acquaintance	0.09	0.08
Number of Migrants	2,463	481

*Respondents are holders of rural registration (*hukou*). The survey was conducted in Fuzhou, Shanghai, Shenyang, Wuhan and Xian during late 2001. Sample frames were assembled using information on distribution of migrants within cities from the 2000 Population Census. After selecting neighborhoods through a proportional population sampling procedure, sample frames were assembled using residents' committee records of migrant households and registers of migrants living on construction sites and held by local by police stations. Very short-term migrants, who lack a residence that falls under the jurisdiction of either of these authorities, are unlikely to have made it into the sample frame.
**A *close family member* is adult sibling or member of nuclear family (e.g., spouse, child, parent). An *extended family member* refers to cousins or other relatives. *Hometown acquaintances* are unrelated, but known by the respondent. Note that migrants may have acquaintances in several categories, so that subcategories of acquaintances will add to more than 100.

Table A.2
Evidence on Educational Attainment of Migrants
from the China Urban Labor Survey (2001)

	Source Community	
	All Provinces	4 RCRE Provinces
Education		
Elementary or Less	0.247	0.220
Some Middle School	0.086	0.096
Middle School	0.485	0.501
Some High School	0.039	0.045
High School	0.120	0.120
Some Post Secondary	0.009	0.011
College	0.010	0.012
Number of Observations	2,463	481

*Source: China Urban Labor Survey (see discussion on note of Table 1).

Table A.3

Reported Age Starting Primary School

Individuals Age 10 to 34 in 2000

Age	Number	Share
4	6	0.002
5	56	0.021
6	530	0.198
7	1336	0.499
8	639	0.239
9	83	0.031
10	18	0.007
11	6	0.002
12	2	0.001
13	2	0.001
14	1	0.000

Source: China Center for Agricultural Policy (CCAP) Data Set, 2000. See de Brauw et al (2002) for a description of the CCAP survey.

Table A.4
Proportion of Individuals Staying in School
by Grade and Primary School Type

	Six Year Primary Schools		Five Year Primary Schools	
Grade	Proportion	N	Proportion	N
2	1.00	1310	1.00	4193
3	1.00	1310	0.99	4186
4	1.00	1296	0.99	4118
5	0.99	1285	0.98	4019
6	0.98	1257	0.91	3904
7	0.95	1211	0.95	3484
8	0.92	1122	0.87	3238
9	0.90	1011	0.43	2712
10	0.47	877	0.68	1134
11	0.95	388	0.84	729
12	0.91	351	0.62	574
13	0.36	305	0.59	329
14	0.91	100	0.81	183
15	0.83	83	0.58	138
16	0.36	61	0.32	74
17	0.40	20	0.21	24
18	0.80	5	0.50	4
19	0.00	3	0.50	2
20	0.00	0	0.00	1

Notes: Proportions are conditional on school enrollment the previous year.
Assumes children start school at age 7 and do not skip.
Source: RCRE Supplemental Survey (2004).

Table A.5
Average Village Characteristics in 1988
by Timing of ID Card Distribution

		Year ID Cards Were Issued			
		prior to 1988	in 1988	after 1988	p-value[1]
Share of Productive Assets Owned by the Village Collective	mean	0.399	0.260	0.246	0.230
	std. dev	0.278	0.205	0.276	
Mean Consumption Per Capita	mean	414.3	367.7	405.1	0.566
	std. dev	154.6	159.6	86.9	
Mean Income Per Capita	mean	627.2	504.2	558.1	0.278
	std. dev	243.3	213.7	162.4	
Cultivable Share of Total Land Area	mean	0.691	0.546	0.512	0.203
	std. dev	0.277	0.268	0.309	
Share in Mountains	mean	0.14	0.23	0.31	0.589
	std. dev	0.36	0.43	0.48	
Share Near a City	mean	0.21	0.04	0.08	0.341
	std. dev	0.43	0.20	0.28	
Cropped Land Gini Ratio	mean	0.21	0.15	0.17	0.024
	std. dev	0.07	0.06	0.05	
Average Household Size	mean	4.40	4.70	4.68	0.311
	std. dev	0.66	0.48	0.53	
Total Village Land	mean	4508	4633	7676	0.512
	std. dev	4694	4676	9401	
Male Share in Population	mean	0.51	0.51	0.50	0.716
	std. dev	0.02	0.02	0.02	
Share of Labor Force Earning Wage Locally	mean	0.27	0.17	0.16	0.317
	std. dev	0.21	0.19	0.22	
Village Population	mean	1646	1288	1501	0.429
	std. dev	1089	537	925	
Village Consumption Per Capita Gini	mean	0.18	0.16	0.16	0.142
	std. dev	0.03	0.03	0.03	
Village Income Per Capita Gini	mean	0.23	0.22	0.21	0.855
	std. dev	0.07	0.05	0.07	
Average Years of Schooling, aged 18-22	mean	8.25	7.57	7.47	0.186
	std. dev	1.73	1.33	1.21	
Share of 15-18 Year Olds Enrolled in High School	mean	0.35	0.33	0.32	0.861
	std. dev	0.47	0.47	0.47	
Observations		14	25	13	

Notes:
1. p-values in column 4 test the hypothesis that the three means are equal.
2. Consumption and income per capita are reported in 1986 RMB Yuan.
3. Sources: RCRE Household and Village Surveys (1986 to 2003), and RCRE Supplemental Surveys (2004).

Appendix Table A.6

Descriptive Statistics for Children Graduating from Middle School

Selected Variables, for Selected Years

	All Years	Year 1987	1990	1993	1996	1999	2002
Individual Level Variables							
Enrolled in High School? (1=yes)	0.43	0.32	0.44	0.41	0.42	0.47	0.47
	(0.50)	(0.47)	(0.50)	(0.49)	(0.49)	(0.50)	(0.50)
Gender (1=male)	0.57	0.58	0.60	0.58	0.53	0.53	0.48
	(0.50)	(0.49)	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)
First Born (1=yes)	0.45	0.47	0.49	0.46	0.52	0.50	0.37
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.48)
Birth Order	1.87	1.92	1.94	1.92	1.76	1.71	1.94
	(1.04)	(1.12)	(1.18)	(1.14)	(1.02)	(0.85)	(0.93)
Household Level Variables							
First Born in Household	0.40	0.28	0.44	0.43	0.38	0.42	0.43
was Male (1=yes)	(0.50)	(0.45)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)
Father's Years of Schooling	6.39	5.44	5.52	6.25	6.47	7.27	6.79
	(3.21)	(3.28)	(3.28)	(3.32)	(3.16)	(3.06)	(3.04)
Mother's Years of Schooling	4.22	3.30	3.07	3.67	4.23	5.02	5.14
	(3.30)	(3.03)	(2.99)	(3.22)	(3.36)	(3.17)	(3.32)
Number of Potential Migrants,	0.46	0.30	0.47	0.45	0.44	0.45	0.54
Household, Male	(0.62)	(0.48)	(0.58)	(0.61)	(0.59)	(0.62)	(0.62)
Number of Potential Migrants,	0.49	0.25	0.51	0.55	0.52	0.47	0.59
Household, Female	(0.71)	(0.48)	(0.69)	(0.81)	(0.81)	(0.66)	(0.79)
Village Level Variables							
Share of Migrants, Village Workforce	0.11	0.03	0.02	0.09	0.13	0.14	0.21
	(0.12)	(0.04)	(0.03)	(0.09)	(0.12)	(0.10)	(0.12)
ln(Village Mean Income Per Capita)	6.42	6.20	6.21	6.22	6.52	6.52	6.66
	(0.39)	(0.32)	(0.35)	(0.31)	(0.32)	(0.38)	(0.31)
ln(Village Mean Wealth Per Capita)	8.81	8.62	8.51	8.69	8.89	8.93	9.07
	(0.55)	(0.48)	(0.57)	(0.48)	(0.50)	(0.50)	(0.45)

Appendix Table A.6 Continued on Next Page

Appendix Table A.6 (Continued)

	All	Year					
	Years	1987	1990	1993	1996	1999	2002
Village Total Land (mu)	5090	4820	5080	4870	5200	5100	5760
	(5710)	(5110)	(5190)	(5500)	(5310)	(6240)	(6460)
Village Cropped Land Per Capita Gini	0.21	0.18	0.18	0.20	0.21	0.23	0.26
	(0.08)	(0.06)	(0.06)	(0.06)	(0.08)	(0.08)	(0.11)
Village Labor Force	861	780	851	867	899	820	948
	(486)	(352)	(419)	(433)	(470)	(487)	(559)
Years Since IDs Issued	7.37	0.33	2.12	4.86	7.87	11.01	13.86
	(5.11)	(1.02)	(1.65)	(2.07)	(2.20)	(2.23)	(2.28)
Cultivable Share of Village Land	0.58	0.60	0.57	0.62	0.55	0.57	0.54
	(0.28)	(0.26)	(0.28)	(0.28)	(0.28)	(0.29)	(0.31)
Forest Share of Village Land	0.15	0.16	0.16	0.15	0.14	0.16	0.19
	(0.27)	(0.27)	(0.25)	(0.27)	(0.25)	(0.28)	(0.30)
Orchards Share of Village Land	0.04	0.02	0.03	0.04	0.07	0.06	0.06
	(0.07)	(0.03)	(0.05)	(0.06)	(0.10)	(0.10)	(0.09)
Aquaculture Share of Village Land	0.04	0.05	0.05	0.04	0.04	0.04	0.05
	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.08)
Share of Households with Non-	0.56	0.66	0.66	0.55	0.57	0.49	0.51
Agricultural Self-Employment Income	(0.28)	(0.27)	(0.27)	(0.27)	(0.25)	(0.28)	(0.26)
Quota Share of Grain Produced	0.09	0.12	0.12	0.08	0.10	0.07	0.03
	(0.08)	(0.10)	(0.09)	(0.08)	(0.08)	(0.07)	(0.05)
Scaled Lagged July-November	0.15	0.02	0.02	0.02	0.04	0.02	0.02
Rainfall Shock, Squared	(2.12)	(0.04)	(0.07)	(0.02)	(0.05)	(0.05)	(0.05)
Number of Observations	3167	158	162	237	262	238	187

Notes: The first column includes descriptive statistics for all years; the second through seventh columns include descriptive statistics for selected years.

Sources: RCRE Supplemental Survey (2004), Annual RCRE Household and Village Surveys (1986-1991, 1993, 1995-2003).

Appendix Table A7
What Factors Determine the Share of Village Residents in the Migrant Network?
First-Stage Regression Using the Sample of Individuals Completing Middle School, 1986-2003

	Dependent Variable: Share of Registered Village Residents Working as Migrants				
Model	1	2	3	4	5
Years Since IDs issued	-0.017*	-0.021**	-0.021**	-0.021**	-0.021**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Years Since IDs Issued Squared	0.007***	0.008***	0.008***	0.008***	0.008***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Years Since IDs Issued Cubed	-0.001**	-0.001***	-0.001***	-0.001***	-0.001***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
$(\text{Years Since IDs Issued})^4/10$	0.002**	0.002***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Village Average Income Per Capita)		0.020	0.020	0.019	0.019
		(0.015)	(0.015)	(0.015)	(0.015)
Total Land in Village (Mu)		0.0004	0.0004	0.0004	0.0004
		0.0003	0.0003	0.0003	0.0003
Cropped Land Gini Coefficient		-0.013	-0.014	-0.011	-0.011
		(0.087)	(0.086)	(0.087)	(0.086)
Size of Village Workforce(/10)		-0.001***	-0.001***	-0.001***	-0.001***
		(0.0002)	(0.0002)	(0.0002)	(0.0002)
Cultivable Share of Village Land		-0.029	-0.030	-0.029	-0.029
		(0.038)	(0.038)	(0.038)	(0.038)
Gender (1=male, 0=female)			-0.001	-0.001	-0.001
			(0.002)	(0.002)	(0.002)
First Born? (1=yes, 0=no)			0.002	0.002	0.002
			(0.003)	(0.003)	(0.003)
First Born in Household is Male? (1=yes, 0=no)			-0.003	-0.003	-0.004
			(0.002)	(0.002)	(0.003)
Father's Years of Schooling				-0.001**	-0.001**
				(0.0004)	(0.0004)
Mother's Years of Schooling				0.0002	0.0002
				(0.0005)	(0.0005)
Number of Potential Migrants, Male					-0.001
					(0.002)
Number of Potential Migrants, Female					0.001
					(0.002)
Number of Observations	3160	3157	3157	3157	3157

Notes: Columns 1 through 5 are the first stage of IV regressions shown in models 1 to 5 of Table 11. .

Appendix Table A.8
Relationship Between Various Definitions of GDP Growth Instrument and Share of Migrants from the Village

	No denominator	Linear in Distance	Quadratic in Distance	$(D\text{-}X)^2$
	(1)	(2)	(3)	(4)
GDP Growth	0.019	-0.829	4.786	0.837***
	(0.050)	(0.708)	(3.590)	(0.303)
Lagged GDP	0.021	1.499**	10.27**	0.490**
Growth	(0.049)	(0.738)	(3.95)	(0.199)

Notes: Each cell represents an individual regression, all of which include village fixed effects and province-year fixed effects. Standard errors clustered at city-year level in parentheses. **-indicates significance at the 5 percent level; ***-indicates significance at the 1 percent level. There are 2394 observations in regressions in row 1 and 2173 observations in row 2.

Appendix Table A.9
Determinants of High School Enrollment using Lagged Growth in Nearest City Instrument Conditional on Completing Middle School, 1993-2003

	Dependent Variable: Enroll in High School Next Year = 1				
Model	1	2	3	4	5
	IV-GMM	IV-GMM	IV-GMM	IV-GMM	IV-GMM
Share of Migrants in Village	-2.179**	-2.638**	-2.181	-2.540*	-2.534*
Labor Force	(1.109)	(1.289)	(1.343)	(1.352)	(1.318)
Gender (1=male)			0.036	0.039*	0.038
			(0.023)	(0.023)	(0.023)
First Born (1=yes)			0.052**	0.026	-0.010
			(0.024)	(0.025)	(0.035)
First Born in Household			-0.065***	-0.075***	-0.054**
was Male (1=yes)			(0.022)	(0.022)	(0.024)
Father's Years of Schooling				0.022***	0.022***
				(0.005)	(0.005)
Mother's Years of Schooling				0.023***	0.023***
				(0.005)	(0.005)
Number of Potential Migrants,					-0.046*
Household, Male					(0.024)
Number of Potential Migrants,					-0.006
Household, Female					(0.020)
ln(Village Mean Income Per Capita)		-0.084	-0.080	-0.068	-0.066
		(0.097)	(0.090)	(0.094)	(0.095)
Village Total Land (/100)		0.002	0.002	0.003	0.003
		(0.002)	(0.002)	(0.002)	(0.002)
Village Cultivable Land Per Capita		0.639	0.501	0.514	0.507
Gini		(0.552)	(0.520)	(0.533)	(0.527)
(Village Labor Force)/10		-0.001	-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Cultivable Share of Village Land		0.170	0.143	0.110	0.112
		(0.205)	(0.189)	(0.199)	(0.198)
Cluster Corrected F statistic	6.079	4.571	4.533	4.539	4.556
Weak instrument robust Anderson-Rubin F statistic	0.33	0.71	0.71	0.31	0.33
Anderson-Rubin p-value	0.566	0.399	0.399	0.581	0.563
Number of Obs.	2,154	2,154	2,154	2,154	2,154

Notes: In parentheses, we show robust standard errors that allow for arbitrary correlation within city-year observations. All regressions control for factors related to village location with village fixed effects, and macroeconomic shocks using province*year fixed effects. All models are estimated using an instrumental variables-generalized method of moments estimator that is efficient in the presence of presence of heteroskedasticity and arbitrary within city-year cluster correlation (see Wooldridge 2002, page 193). Weak instrument robust Anderson-Rubin F statistics are calculated with a minimum distance estimator suggested by Finlay and Magnusson (2011). *-indicates significance at the 10 percent level; **- indicates significance at the 5 percent level; ***- indicates significance at the 1 percent level.

Appendix Table A.10
Determinants of High School Enrollment using Contemporaneous Growth in Nearest City Instrument
Conditional on Completing Middle School, 1991-2003

	Dependent Variable: Enroll in High School Next Year = 1				
Model	1	2	3	4	5
	IV-GMM	IV-GMM	IV-GMM	IV-GMM	IV-GMM
Share of Migrants in Village Labor Force	-2.162***	-2.275***	-2.135**	-2.166***	-2.168***
	(0.733)	(0.840)	(0.835)	(0.700)	(0.699)
Gender (1=male)			0.026	0.030	0.029
			(0.022)	(0.022)	(0.022)
First Born (1=yes)			0.047**	0.022	-0.011
			(0.023)	(0.023)	(0.033)
First Born in Household was Male (1=yes)			-0.062***	-0.072***	-0.056**
			(0.021)	(0.021)	(0.023)
Father's Years of Schooling				0.020***	0.020***
				(0.005)	(0.005)
Mother's Years of Schooling				0.024***	0.023***
				(0.005)	(0.005)
Number of Potential Migrants, Household, Male					-0.040*
					(0.023)
Number of Potential Migrants, Household, Female					-0.008
					(0.019)
ln(Village Mean Income Per Capita)		0.006	0.006	0.016	0.019
		(0.093)	(0.091)	(0.090)	(0.091)
Village Total Land (/100)		0.001	0.001	0.001	0.001
		(0.002)	(0.002)	(0.002)	(0.002)
Village Cultivable Land Per Capita Gini		0.316	0.272	0.223	0.209
		(0.412)	(0.399)	(0.390)	(0.390)
(Village Labor Force)/10		-0.001	-0.001	-0.001	-0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Cultivable Share of Village Land		0.152	0.147	0.091	0.095
		(0.175)	(0.169)	(0.165)	(0.166)
Cluster Corrected F statistic	7.636	6.626	6.647	6.846	6.840
Weak Instrument Robust Anderson-Rubin F-Statistic	4.55	4.38	3.88	3.89	3.92
Anderson-Rubin p-value	0.033	0.036	0.049	0.049	0.048
Number of Obs.	2,372	2,372	2,372	2,372	2,372

Notes: In parentheses, we show robust standard errors that allow for arbitrary correlation within city-year observations. All regressions control for factors related to village location with village fixed effects, and macroeconomic shocks using province*year fixed effects. All models are estimated using an instrumental variables-generalized method of moments estimator that is efficient in the presence of presence of heteroskedasticity and arbitrary within city-year cluster correlation (see Wooldridge 2002, page 193). Weak instrument robust Anderson-Rubin F statistics are calculated with a minimum distance estimator suggested by Finlay and Magnusson (2011). *-indicates significance at the 10 percent level; **- indicates significance at the 5 percent level; ***- indicates significance at the 1 percent level.

Appendix Table A.11
Returns to Education Among Migrants from RCRE Villages in 2003
Heckman Selection Models

	Model 1		Model 2	
	ln(Daily Migrant Wage)	Migrant? (1= Yes)	ln(Daily Migrant Wage)	Migrant? (1= Yes)
Years of Schooling	0.029	-0.007	--	--
	(0.012)	(0.014)		
0<=Years of Schooling <9	--	--	0.040	0.072
			(0.019)	(0.022)
9<=Years of Schooling <12	--	--	0.003	-0.126
			(0.034)	(0.036)
Years of Schooling>=12	--	--	0.047	-0.029
			(0.059)	(0.068)
Age	0.146	0.285	0.149	0.286
	(0.041)	(0.043)	(0.034)	(0.042)
Age Squared	-0.003	-0.005	-0.002	-0.005
	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.213	0.311	0.217	0.307
	(0.053)	(0.061)	(0.052)	(0.061)
Fathers Years of Education	-0.013	-0.0283	-0.012	-0.027
	(0.010)	(0.011)	(0.009)	(0.011)
Mothers Years of Education	0.008	-0.005	0.008	-0.005
	(0.010)	(0.012)	(0.010)	(0.011)
Household Size	--	-0.010	--	-0.020
		(0.044)		(0.044)
Number of Adult Laborers	--	0.054	--	0.057
		(0.046)		(0.046)
Household Land Per Capita	--	-0.115	--	-0.126
		(0.052)		(0.052)
Number of Elderly in Household	--	-0.054	--	-0.045
		(0.038)		(0.038)
Dependency Ratio	--	-0.079	--	-0.087
		(0.178)		(0.179)
Male/Female Ratio	--	-0.354	--	-0.317
		(0.202)		(0.202)
Number of Children Under 5	--	-0.197	--	-0.203
		(0.068)		(0.069)
Number of Observations	3880		3880	
Censored Observations	3101		2101	
Uncensored Observations	779		779	

Notes: Individual information necessary to estimate daily returns to education from migrant employment are only available for the 2003 survey. We estimate returns to education in migrant employment for children of the household head and spouse who are under 50 and over 15 years of age. Standard errors, clustered at the village, are shown in parentheses.